PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME LXIX

1930

PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
1930

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MINUTES OF THE MEETINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

DURING 1930

Stated Meeting, January 3, 1930

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President in the Chair

The decease was announced of the following member:

William M. Meigs, A.M., M.D., at Philadelphia, Pa., December 30, 1929, æt. 77.

Clarence E. McClung read a paper on "A Working Model of a Modern University" which was discussed by Messrs. Conklin, Calvert, Donaldson, Snyder, Dercum, Lingelbach, Ames, Goodwin, and Messrs. Linten, Hatfield and McClenahan, guests.

Stated Meeting, February 7, 1930

ALBERT P. BRUBAKER, A.M., M.D., LL.D., in the Chair.

The decease was announced of the following members:

Thomas Harvey Dougherty, B.S., at Philadelphia, Pa., October 25, 1929.

Francis Rawle, A.M., LL.B., at Philadelphia, Pa., January 28, 1930, æt. 83.

Isaiah Bowman read a paper on "Antarctica" which was illustrated by lantern slides and maps and discussed by Messrs. Bryant, Scott and Hamilton Rice, a guest.

Dr. Bowman's paper was broadcast over an extensive network arranged by the National Broadcasting Company. The address was also broadcast by KDKA, Pittsburgh, a

short wave station, direct to Admiral Richard E. Byrd, Sir Hubert Wilkins and Sir Douglas Mawson in Antarctica.

A radioed reply from Admiral Byrd was received.

The following papers were read by title:

"Upper Cretaceous Dinosaur Faunas of North America," by Loris S. Russell. Introduced by William B. Scott.

"Studies on Weathering and Soil Formation in Tropical High Altitudes," by Maurice W. Senstius. Introduced by William H. Hobbs.

The Chair announced a vacancy on the Committee on Hall.

Pending nominations were read.

Stated Meeting, March 7, 1930

Eli Kirk Price, A.B., LL.B., in the Chair

The decease was announced of the following members:

Richard Mills Pearce, Jr., M.D., Sc.D., at New York, N. Y., February 16, 1930, æt. 55.

Arthur Twining Hadley, M.A., Ph.D., LL.D., at Kobe, Japan, March 5, 1930, æt. 73.

Alexander C. Abbott was appointed to fill the vacancy on the Committee on Hall.

The Committee on Nominations of Officers made its report.

The Committee on Library presented the following resolution:

Resolved that the Library Committee recommend to the Society that inasmuch as one of the principal functions of the Society is the efficient administration of its library, when the definitive plans for the new building are under consideration the Building Committee request the advice and criticism of Mr. John Ashhurst or other experienced librarians, in order that the plans for arrangement and equipment of its rooms, including those for study and for administration, conform to the best standards of library practice.

Mr. Price reported on the progress of the campaign.

Messrs. Alba B. Johnson and Clarence E. McClung also made brief addresses concerning the campaign.

Special Meeting, March 26, 1930

Eli Kirk Price, A.B., LL.B., in the Chair.

James M. Beck read a paper on "The Changed Conception of the Constitution" which was broadcast by the National Broadcasting Company.

General Stated Meeting, April 24, 25, 26, 1930

Thursday Afternoon, April 24th

Opening Session, 2 o'clock

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

Clinton J. Davisson, recently elected member, subscribed the Laws and was admitted into the Society.

The decease was announced of the following members:

William Howard Taft, B.A., LL.D., D.C.L., at Washington D. C., March 8, 1930, æt. 73.

Henry Chapman Mercer, A.B., Sc.D., at Doylestown, Pa., March 9, 1930, æt. 74.

Stephen Alfred Forbes, Ph.D., LL.D., at Urbana, Ill., March 13, 1930, æt. 85.

Martin Grove Brumbaugh, M.D., M.S., A.M., Ph.D., LL.D., Litt.D., L.H.D., at Philadelphia, Pa., March 14, 1930, æt. 67.

Arthur James Balfour, K.G., O.M., LL.D., D.Cl., at London, England, March 19, 1930.

Horace Howard Furness, Jr., A.B., Litt.D., LL.D., at Philadelphia, Pa., April 15, 1930, æt. 65.

Herman Snellen, Jr., at Utrecht, Netherlands.

The following papers were read:

"The Wave Properties of Electrons," by Clinton J. Davisson, Bell Telephone Laboratories, New York. Discussed by Dr. Goodspeed.

"The Chromolinoscope Revived," by Herbert E. Ives, Bell Telephone Laboratories, New York. Discussed by Dr. Goodspeed.

Dr. Goodspeed presented a brief abstract of a possible device by Mr. Clayton Williams, the purpose of which is to project pictures stereoscopically. Dr. Goodspeed asked Dr. Ives if the plan seemed practicable and Dr. Ives replied that in its present form the device was not practicable, stating clearly why.

"Parallel Connections of Identical Electrical Nets," by Arthur E. Kennelly, Professor of Electrical Engineering, Harvard University.

"The Present Status of International Law," by Clyde Eagleton, Associate Professor of Government, Washington Square College, New York University. Introduced by Emory R. Johnson.

"Present Phases of Railroad Consolidation," by Emory R. Johnson, Professor of Transportation and Commerce, University of Pennsylvania.

"The Story of Glozel—A Chapter in Credulity," by David Riesman, Professor of Clinical Medicine, University of Pennsylvania. Introduced by Francis X. Dercum.

"On Some Silurian Fishes from Norway," by William B. Scott, Professor of Geology, Princeton University.

"The Stratigraphy and Palæontology of the Paleocene of Northeastern Park County, Wyoming," by Glenn L. Jepsen, Princeton University, Introduced by William J. Sinclair.

"Bearing of Titanothere Researches on the Principles of Descent and Adaptive Radiation of the Mammals," by Henry Fairfield Osborn, American Museum of Natural History, Dr. Osborn's paper was read by William B. Scott.

Friday, April 25th

Executive Session—9:30 o'clock

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President in the Chair.

The President delivered his annual report and appointed the Committees on Nominations and General Meeting.

The Society proceeded to an election of Officers and Members.

The Tellers subsequently reported that the following officers and members had been duly elected:

President

Francis X. Dercum

Vice-Presidents

James H. Breasted
Elihu Thomson
Henry N. Russell

Secretaries

Arthur W. Goodspeed
John A. Miller

Curator

Albert P. Brubaker

Treasurer

Eli Kirk Price

Councillors

(To serve for three years)

William B. Scott
John F. Lewis
Dana C. Munro
Edwin B. Wilson

Members

Norman L. Bowen
Richard E. Byrd
Cyrus H. K. Curtis
Harvey Cushing
Francis I. duPont
Raymond B. Fosdick
Thomas S. Gates
William Guggenheim
Archer M. Huntington
Merkel H. Jacobs
Charles F. Kettering
Alfred L. Loomis
James B. Scott
Frederick Tilney
Alexander Wetmore

Foreign Members

Albert Einstein
John Stanley Plaskett
Sir Hubert Wilkins

Morning Session—10 o'clock

CLARENCE E. MCCLUNG, A.M., Ph.D., in the Chair

The following papers were read:

"Impulse Rythms in Sensory and Motor Nerve Fibres," by Detlev W. Bronk, The Eldridge R. Johnson Foundation for Medical Physics, University of Pennsylvania. Introduced by Henry H. Donaldson.

"Identical Twins as Biological Controls in Educational and Other Human Problems," by Albert F. Blakeslee, Assistant Director in Plant Genetics, Carnegie Station for Experimental Evolution, Cold Spring Harbor, and Howard J. Banker.

"An Electro-Chemical Interpretation of Memory," by George W. Crile, Director of the Cleveland Clinic and of the Cleveland Clinic Hospital.

"The Natural Chemical Regulation of Growth by Increase in Cell Number," by Frederick S. Hammett, Director of Research, The Research Institute of the Lankenau Hospital, Philadelphia. Introduced by Francis X. Dercum.

"Lengthened Growing periods and Continuous Growth," by Daniel T. MacDougal, Research Associate, Carnegie Institution of Washington. (Read by title.)

"The Toxicity of Molecules and Ions to the White Lupine," by Rodney H. True, Professor of Botany, University of Pennsylvania. (Read by title.)

"The Menace of Disease in American Forests," by Harlan H. York, Botanical Laboratory, University of Pennsylvania. Introduced by Rodney H. True.

"Geography of American Peppers," by William Trelease, Professor of Botany, University of Illinois. Discussed by Dr. Shull.

"The Earliest Civilizations of the Near East," by Ephraim Avigdor Speiser, Assistant Professor of Semitic Languages, University of Pennsylvania. Introduced by Cyrus Adler.

"Vergil after Two Thousand Years," by John C. Rolfe, Professor of Latin, University of Pennsylvania.

Afternoon Session—2 o'clock

FRANK P. GRAVES, A.M., Ph.D., Litt.D., L.H.D., LL.D., in the Chair

A Symposium on "The Outlook for Higher Education in the United States" was presented by Abraham Flexner, Educator; F. J. E. Woodbridge, Dean of the faculties of Political Science, Philosophy, pure Science and fine Arts, Columbia University; Frank Aydelotte, President, Swarthmore College.

The Symposium was discussed by Drs. Adler, Cheyney, W. A. Noyes and two guests.

Friday Evening Lecture

William F. Albright, Professor of Semitic Languages, Johns Hopkins University, formerly Director of the American School of Oriental Research in Jerusalem, spoke on "A Millennium of Biblical History in the Light of Recent Excavations."

Saturday, April 25th

Morning Session—10 o'clock

Henry H. Donaldson, A.B., Ph.D., Sc.D.,
in the Chair

The following papers were read:

"The Present Trends in American Anthropology," by Fay-Cooper Cole, Professor of Anthropology, University of Chicago. Introduced by Aleš Hrdlička. Discussed by Dr. Hrdlička.

"Tertiary Man—Critical Discussion," by Aleš Hrdlička, Curator, Division of Physical Anthropology, U. S. National Museum, Smithsonian Institution. Discussed by a guest.

"Recent Archæological Discoveries in the Philippines and Their Bearing on the Pre-history of Eastern Asia," by Roland B. Dixon, Professor of Anthropology, Harvard University. (Read by title.)

"Recent Progress in the Field of Old World Pre-history," by George Grant MacCurdy, Research Associate in Pre-historic Archæology and Curator of the Anthropological Collections, Yale University. Discussed by Dr. Hrdlička and a guest.

"Fundamental Problems of American Prehistory," by Alfred V. Kidder, Carnegie Institution of Washington. Introduced by John C. Merriam.

"The Kinetics of Bioluminescent Reactions of Short Duration," by E. Newton Harvey, Professor of Physiology, Princeton University, and Peter A. Snell.

"The Nervous System in Relation to Movement in Annelid Worms," by J. H. Ashworth, Professor of

Natural History, University of Edinburgh. Introduced by Clarence E. McClung.

"The Surface Precipitation Reaction of Living Cells," by L. V. Heilbrunn, Associate Professor of Zoology, University of Pennsylvania. Introduced by Clarence E. McClung.

"Heights in the Chromosphere Derived from Eclipse Spectra," by Samuel A. Mitchell, Professor of Astronomy, Director of the Leander McCormick Observatory, University of Virginia.

Afternoon Session—2 o'clock

ELIHU THOMSON, A.M., Ph.D., D.Sc., LL.D., Vice-President in the Chair

William F. Albright, E. Newton Harvey and Edwin W. Rice, Jr., recently elected members, subscribed the Laws and were admitted into the Society.

A Symposium on "Astronomy and Astrophysics" was presented as follows:

"An Introductory Statement," by John A. Miller, Research Professor of Astronomy, Swarthmore College, Director Sproul Observatory.

"The Measurements of Time and Changes in the Rotation of the Earth," by Ernest W. Brown, Sterling Professor of Mathematics, Yale University.

"The Structure and Rotation of the Galaxy," by John S. Plaskett, Director Dominion Astrophysical Observatory, Victoria, B. C.

"The Physics of a Star," by John Q. Stewart, Associate Professor of Astronomical Physics, Princeton University. Introduced by John A. Miller.

"Progress in Extra-Galactic Explorations," by Harlow Shapley, Professor of Astronomy, Harvard University, and Director Harvard Observatory.

Saturday Evening

The Annual Dinner was held at the Bellevue Stratford Hotel.

President Dercum introduced John H. Finley who presided and the toasts responded to were as follows:

"The Service of Science to Mankind," Edwin G. Conklin
"Seeking Useful Knowledge," Sir Hubert Wilkins
"Promoting Useful Knowledge," James R. Angell

Stated Meeting, November 7, 1930

Francis X. Dercum, M.D., Ph.D., Sc.D., President in the Chair

Cyrus H. K. Curtis, Francis I. duPont and Merkel H. Jacobs recently elected members, subscribed the Laws and were admitted into the society.

Dr. Goodspeed announced that the Carnegie Corporation of New York had appropriated $100,000 to the Society for the maintenance of its Library, in accordance with the following resolution passed by the Carnegie Corporation on October 15, 1930:

Resolved that, from the amount available for appropriation the sum of one hundred thousand dollars ($100,000) be, and hereby is, appropriated to the American Philosophical Society for the maintenace of its Library.

The decease was announced of the following members:

Comte Hyacinth de Charencey, at Paris, France, in 1917.

Sir William Turner Thistleton-Dyer, K.C.M.G., M.A., S.C., Ph.D., LL.D., December 28, 1928.

Gregory B. Keen, A.M., LL.D., at Philadelphia, Pa., April 30, 1930, æt. 86.

Fridtjof Nansen, Sc.D., D.C.L., LL.D., Ph.D., Kt., June 25, 1930, æt. 69.

Kuno Francke, Ph.D., Litt.D., LL.D., at Cambridge, Mass., June 25, 1930, æt. 75.

Harvey W. Wiley, A.M., S.B., M.D., Ph.D., Sc.D., LL.D., at Washington, D. C., June 30, 1930, æt. 85.

William Diller Matthew, A.M., Ph.D., at San Francisco, Calif., September 24, 1930, æt. 60.

Adolph Engler, Ph.D., Med.D.h.c., in Germany, October 10, 1930, æt. 87.

Aleš Hrdlička, read a paper on "The Search for Man's Migrations in Alaska" which was illustrated by lantern slides and discussed by John F. Lewis and Mr. Schor, a guest.

Monroe B. Snyder presented an informal communication on *The Clarke Atomic Triads.*

The attention of the members was called to a volume being published by the Princeton University Press entitled: Cope: "Master Naturalist" by Henry Fairfield Osborn.

A pamphlet entitled "The Unconstitutionalities of the Volstead Act with a Substitution Plan" by Joseph Battaglia was presented and ordered to be filed.

On motion the amendment to the Laws proposed by the American Council of Learned Societies was ratified.

On motion of Secretary Goodspeed, William E. Lingelbach was appointed to represent the American Philosophical Society on the American Council of Learned Societies for the next three years.

The following amendment to the Laws was proposed:

In Chapter I, Section I, strike out "At such an election no more members residing within the United States shall be elected than will make the total number of such membership 400" and insert in lieu thereof "At such an election ten more members than the number of those deceased during the previous year may be elected each year until the total membership of the United States is 500 and two more foreign members than the number of those deceased may be elected each year until the total foreign membership is 60."

The Curator obtained permission to remove the American Philosophical Society Poinsett Collection from the Archæological Department of the Academy of Natural Sciences,

Philadelphia, and to place it on deposit in the new Museum of the University of Pennsylvania.

Stated Meeting, December 5, 1930

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President, in the Chair

The decease was announced of the following members:

J. Edward Whitfield, Ph.D., at Philadelphia, Pa., November 4, 1930, æt. 71.

Andrew S. McCreath, at Harrisburg, Pa., November 26, 1930, æt. 82.

Frederick Tilney read a paper on "Structural Relations of the Brain underlying the Reactions of the Organism to the Environment" which was illustrated by lantern slides and discussed by Drs. Donaldson and Schaeffer.

The following paper was read by title:

"Non-Living and Living Matter," by Francis X. Dercum.

The minutes of the Stated Meeting of the Council, held on November 21st, were submitted and the change in the wording of the amendment to the Laws suggested was approved to read as follows:

Twenty-five members may be elected each year until the total membership of the United States shall be 500 and five foreign members may be elected each year until the total foreign membership shall be 60, these numbers to be kept constant thereafter by electing each year an appropriate number of new members.

The Annual Report of the Girard Trust Company, Trustees of the Building Fund was presented and on motion was referred to the Committee on Audit.

The Treasurer presented the following resolution which was approved by the Society:

RESOLVED that the Treasurer be authorized to transfer from the principal to the income account of the General Fund such amount, as may be found necessary to cover any deficit in the operating

account of the Society at the close of the current year, and, if necessary, to withdraw from the Pennsylvania Company for Insurances on Lives and Granting Annuities and sell any security, constituting part of the investments of that Fund, that may be required for the purpose.

The following resolution was unanimously adapted by the Society:

The American Philosophical Society having learned with deep regret of the death of the wife of our Secretary, Dr. John A. Miller, hereby expresses to Doctor Miller its heartfelt sympathy in his bereavement.

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VOL. 69 1930 NO. 1

WHY THE 200-INCH TELESCOPE

By ELIHU THOMSON

(Read December 6, 1929)

THE object of the society which I now address, namely, The American Philosophical Society, the oldest society of the kind in America, and which has existed for about two hundred years, is to coördinate knowledge in all the different fields, to seek expansion of that knowledge in new directions, and to assist in the progress obtained by improving the facilities, not only in the study of science, but in the humanities and in all phases of human effort, leading to a better understanding of our life on this earth. It is here that the leaders in investigation, the explorers, the advance thinkers, may present their results, and have them recorded. This, indeed, is philosophy in its broadest sense.

As civilization spreads, as populations increase, there comes, naturally, the need of a widened sphere of activity, more resources to conserve the recorded results of the work of the eminent men, and to make adequate provision for the inevitable expansion of the work in the present and the future.

The society is fortunate, at this time, in having started on a period of expansion which will eventuate, we hope, in realizing the objects which I have just pointed out. Fortu-

nately, too, from Franklin's time, The American Philosophical Society has been uninterruptedly active and has attracted to its meetings and programs leaders of thought from all parts of our great and growing nation, as well as from abroad. Its limited quarters are now outgrown, and it must seek its expansion elsewhere than in this hall. I need not dwell on the details of this enlarged program, which have been carefully worked upon by the officers of the society, and which it is earnestly hoped can be carried through successfully.

My present task is quite different, although, in a sense, there is an analogy. The astronomers have greatly increased their facilities, extended their observations, and have approached the need for more effectual instrumental equipment in the exploration of the great universe in which we exist. I may quote here a paragraph from an article which was published in *Harper's Magazine* for November, 1929. It was from the pen of Dr. George Ellery Hale, honorary director of the Mount Wilson Observatory, near Pasadena. He begins his text with these words:

"Astronomers, like other men, spend most of their lives in hard and often tedious routine work. They are, however, sometimes fortunate enough to take part in a great adventure, and it is of such an adventure that I am now writing."

The title of the article of which the quotation just made is the opening paragraph in "Building the 200-inch Telescope." I commend to anyone who is interested the reading of that article, because Dr. Hale has presented in a brief form, from the astronomers' standpoint, the reasons for and the advantages to accrue from the possession of the proposed giant instrument.

It will not be my function, in dealing with the subject of the 200-inch telescope, to treat it as an astronomer, nor shall I treat it as a designer of a huge instrument, surpassing in magnitude anything before undertaken, would treat it. I shall not compare it with a battleship in cost, although it will involve a large expenditure, but what I will have to say

will concern the heart and soul of the great instrument, so to speak, namely, the mirror which reflects the light from the distant celestial objects and brings it either to the photographic plate or to the human eye.

There exists at the Mount Wilson Observatory, near Pasadena, the largest telescopic instrument in the world, usually spoken of as the 100-inch. The mirror, which, of course, is the most important part of the instrument, is made of a great block or disk of glass, about one hundred inches in diameter, and this block is about thirteen inches thick and weighs, I think, about four or five tons. It took a long time to melt the glass; indeed it was difficult to find any glass-maker to undertake to make that glass disk, there were so many uncertainties. Three large pots of glass were poured into a mould and cooled through months of gradual loss of heat so as to anneal the glass and prevent its going to pieces by uneven contraction and expansion. But that was only a portion of the difficulty. Once obtained, the glass had to be worked into the form of a disk having a slightly concave surface, and this surface had to be shaped with the greatest accuracy to the form of what is known as a paraboloid of revolution, so that the light of a distant star for example would be brought to a single point, or as near to a point as possible. The light of a star falls on every part of the surface of the mirror one hundred inches across and from that face must be reflected so as to reach a focus many feet away, all parts of the extended surface coöperating to direct the light to that distant focal point. The least divergence from the proper path of the light ray from any part of the surface is destructive; the image is imperfect, blurred, more or less indistinct; the fine definition, as it is termed, is lost.

I saw the 100-inch mirror under preparation at Pasadena by Dr. George W. Ritchey. The grinding and polishing is nothing as compared with the time it takes to figure or give final exact shape to the mirror surface; that is, to give to it its final form within a few millionths of an inch. This is to say that the accuracy of the surface must be exceedingly

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great, inconceivably great for most people to visualize. When the surface is being formed by polishing it by a gently moving polisher, the glass is slightly heated and distorted. This is enough to make it necessary to stop the work for the day and put the mirror "to bed," so to speak, under five inches or more of felt covering until the next day.

The next day the first operation is to uncover it and make tests by rays of light and optical means as to its form in different parts or zones, as they are called. These tests completed, the polishing goes on with the greatest care not to overrun or spoil the work already done, until at last, after about two years of this process with the 100-inch, the desired figure or shape was attained; be it remembered, not by grinding, not because one polishes the surface, but by polishing in just those places and to just that extent as will remove a very small amount of the glass and give it the paraboloidal final figure. Thus arose into being the present 100-inch mirror, the largest in the world and the basis for the largest existing telescope.

Popularly, one might imagine that the making of the mirror itself completes the work, but it does not. The mirror has then to be mounted in a balanced position so that it is not distorted by changing its position. This involves an elaborate scheme in itself. It has to be mounted so as to be turned in any direction facing the different parts of the heavens without being bent or even slightly distorted; it has to have a very thin coat of silver deposited on its front surface, because it is not like the ordinary looking-glass mirrors; one does not look through it, but at it. The light falls on its front surface and is reflected from the exceedingly thin, polished film of silver which has been deposited on that front surface by chemical means. But that is not all, by any means; there is a great deal more. Such a mirror, however perfect in its make-up, its construction, in its mounting, in its silvering, is still subject to distortion whenever the temperature of the air around it changes. The face and edges of the mirror naturally change their temperature more rap-

idly than the body, and glass being a substance expanding by heat, the change of one part of the mirror in relation to another part of even a very few degrees practically destroys the accuracy of its reflection and the accuracy of the images which man is seeking to examine visually or to photograph. So much so is this the case, that there always must be, and is, a consideration of the coming night as to temperature, and an effort is made to guess ahead of time what that temperature may be, and means are provided for giving the mirror the temperature which may have been assumed; mistakenly, of course, in many instances.

I am told that the 100-inch glass mirror is rarely used with its full aperture or with a full opening or full face (the 100-inch circle) but its edge is often cut down more or less so as to lessen its effectual size, according to the sharpness of the images.

It is difficult, indeed, to convey in a few words the necessity for exceeding accuracy of shape or figure of the surfaces of these mirrors and to make it understood how little a disturbance will throw them off or make them ineffective for the work for which they are designed. There is a number of fairly large glass mirrors in existence in the astronomical observatories of the world, but they all suffer from the same disadvantages of glass expansion which I have tried, in as brief as possible a way, to point out.

It is now about thirty years since I made the first experiment, comparing a small slab of fused quartz, or fused silica, with a similar slab of glass, as preliminary to further work. I formed on the surface of each of these, a slightly concave surface, and then used well-known optical tests to show whether the figure was maintained under different conditions. The experiment was, naturally, imperfect, but I felt sure of the result. On having the two mounted so that I could have a distinct and clear image of a small artificial star or point of light when used with an eye-piece as a telescope is used, I found that by instantaneous application of a moderate heat or a small flame on the back of the glass slab, the image went

immediately all to pieces, as we may say; that is, it scattered, all definition lost. A similar treatment of the quartz slab showed very little change and not until the back of the quartz had become quite hot was there a semblance of the disturbance that occurred with the glass.

This experiment, modest as it was, convinced me that there was one material suitable for the making of astronomical reflectors that would avoid many of the difficulties of construction and operation inherent with the glass mirror telescopes. I have kept the matter in mind, and only in recent years have I been able to extend the work to include mirrors of such size as would be actually useful in astronomical work. Dr. George E. Hale, himself, was one of the first to ask for blocks of fused quartz from which mirrors could be made.

What, then, is our task at the Thomson Research Laboratory at Lynn, of which I am Director? This is a laboratory of the General Electric Company used for research. Is our task to provide the funds for the work? No. They are provided by certain funds which exist and which are devoted to this purpose. Is our task, then, to design and build the telescope structure of steel framing; admirable and huge mechanism as it will be? No, we have not that task; others will take care of that part of it. Then what is our task? The answer is simple; it is to provide the mirror, and this mirror body will have to include in it two new pioneer characteristics. The first, its great size, far beyond anything before contemplated. It will be twice the diameter of the existing 100-inch mirror at Mount Wilson and have, therefore, four times the surface, and be very much thicker. It may be six to eight times the weight of the glass mirror at Mount Wilson, or say twenty-five to thirty tons in weight. The second and more important pioneer character is in the material used for such a mirror. It is not of metal, as the great Lord Ross instrument was, speculum metal, six feet in diameter; it is not to be of glass, as in the most recently constructed instruments of large size. Glass, as I have pointed out, gives great difficulties. It is very difficult to melt the

glass, get it uniform and cast it, with the subsequent slow cooling and annealing through months of time, to avoid its splitting or fracturing by uneven cooling or having within it uneven structure. It is far more difficult to shape glass to the accurate figures demanded, as the building of the 100-inch mirror showed clearly. It is even difficult to use, on account of the deformity it undergoes by changes of external temperature, disturbing the direction of the light reflected from parts of its surface, and spoiling its performance.

No, it is not glass that we shall use, but fused silica, or melted quartz, melted in an electric furnace at between seventeen hundred degrees and eighteen hundred degrees Centigrade, which means more than three thousand degrees Fahrenheit, or about the melting point of platinum, a metal difficult to melt, as is well known, and which does not melt in ordinary flames or furnaces. The method we shall use, and which we are using, in fact, on a small scale with great success, is, in general terms, one devised quite a number of years ago, and which consists in first melting a mass of good clean quartz sand in a circular mould in an electric furnace, and obtaining thereby a disk or thick slab of melted quartz sand. This is, indeed, fused quartz, but full of tiny bubbles, which tend to make it lighter, but the melted sand has all the desirable properties of the solid fused quartz itself.

Now, what is the most striking and important of those desirable properties? Simply this, that it is a rigid hard material, almost expansionless by change of temperature. A bar of it a meter long, or a little over a yard long, if raised in temperature from ordinary room temperature through one thousand degrees Centigrade, or about eighteen hundred degrees Fahrenheit (a very bright heat and one which is very near the melting point of gold), expands only a half a millimeter, or about one-fiftieth of an inch. Considering this great range of eighteen hundred degrees Fahrenheit, it can well be understood that under ordinary temperature changes fused quartz practically does not expand or contract. It retains its dimensions.

Now if a mirror is made of such material it retains its dimensions and does not bother the worker in either producing it or using it. There is not the long task of polishing it and letting it cool between the efforts from day to day.

But I have only spoken of the foundation or base body of the mirror. I have not said anything about the second stage in its production. This comparatively rough, bubble-filled mass of melted sand, which is the underlying disk, has to be provided with a surface layer, more or less thick, of clear glass-like fused quartz, or silica glass. It will have the same general properties, so far as expansion goes, and, therefore, will suit the purpose very well united to the sand backing. The first efforts were made by melting on to the fused sand backing slabs of clear quartz made in a different way, and the results were fairly successful. Fair mirrors could be made in that way, but at the suggestion of one of our skilled workers, an experiment was made of feeding into an oxy-hydrogen blowpipe flame, granulated or finely powdered crystal quartz (rock crystal) of high quality, and immediately it was found that a coating of clear quartz could thus be deposited upon any other piece of quartz. When oxygen and hydrogen are burned together in a jet, the temperature of the flame is high enough to fuse or melt silica, or quartz. By raining down through such a flame, the granulated crystal quartz is received on a surface much as ice deposits in clear layers on objects during a sleet storm. In illustration, I often use the analogy, only adding that ours is a high temperature sleet storm, with quartz deposited in clear layers instead of ice.

Extending this to a mirror, it was found that under proper precautions of temperature, the surface might be glazed with clear fused silica to any ordinary desired thickness, by the simple process of introducing high grade silica into a flame of such temperature as will melt readily the particles of silica as they pass through it towards the surface which receives it. In this case, of course, it is the surface of the mirror to be, as composed of the fused sand backing, which in the way I

have outlined is covered with a layer of beautiful, clear, transparent fused quartz. When I speak of fused quartz or fused silica, or silica glass, the same material is meant and the terms are used interchangeably.

It has taken considerable effort and time, and some money, to arrive at this stage in the operation, but the results are justifying themselves right along, and were it not so I should not have been ready or willing to tell you of the progress towards the making of the 200-inch mirror. We now feel that our task is indeed an enlargement of the scale of our operations. We do not, however, minimize the obstacles which will have to be overcome in enlarging to the great scale on which we must work when producing the 200-inch mirror. It is a gigantic thing in itself, but such a gift to science is a prize that is worth working for.

It has, of course, been necessary in doing this work and carrying it along, and the necessity will continue to the end of the accomplishment, that competent, enthusiastic coadjustors shall exist to take care of the very varied things which present themselves. They must, indeed, be both competent and enthusiastic. The work has just begun, and mirror blanks suitable for use in astronomical telescopes up to about two feet in diameter are produced and producible. These have all the desirable qualities, which, of course, will be those of the great mirror. We cannot speak of taking only months to produce the large mirror; we must speak of years. It may be two or three years before the large mirror is turned out, but every product of our work is itself an acquisition for astronomy, whether it be two feet, three feet, five feet, six feet mirrors, and we will go to our goal by steps. Every one of these interim products will be a useful and desirable thing for the astronomer; in fact, the construction of the 200-inch mirror involves the production of at least two or three others of less size to be used with it, in several combinations.

The process of actually shaping; the grinding, polishing and figuring of these quartz mirrors, is practically the same

as has been used in the making of the glass mirrors hitherto, and will be done probably at Pasadena under the care of the Mount Wilson Observatory optical staff, but with the great advantage that the work does not have to stop for equalization of temperature in the mass of the mirror as with glass, but can go on steadily, uninterruptedly, occupying days instead of years, and experience shows us that the material lends itself more readily than glass to the work of the optician.

The mounting and use of the great mirror will be the same, except for a great enlargement of scale, as it would be with a glass mirror, but there will not be the disadvantage of changes of temperature disturbing the accuracy of the work or its possibility. The fearsome process of using the sun itself as the object of investigation, for example, will not trouble with a mirror of fused quartz, as it would with a glass mirror. It is, of course, necessary to establish such an instrument in the best or steadiest air that can be found, little subject to currents of different temperature which disturb the passage of the light to and from the mirror in the atmosphere itself. Proper sites are even now being sought for the mounting of the great instrument which is contemplated.

The mirror blank itself will probably be made at the River Works of the General Electric Company at West Lynn, or nearby, and I wish to mention here the fact that the undertaking is carried on at the works of the General Electric Company without any idea of profit, but merely at the cost of labor and material, and a free gift is made by that company to the enterprise, of the engineering skill and existing laboratory equipment useful to the attainment of the end in view.

Naturally, the placing of the giant instrument is a problem in itself which we can leave to the astronomers. We can leave, also, to the astronomers what may be expected from the use of the giant mirror in the completed telescope, a great engineering structure itself, except to say in general

that its light gathering power will be probably not less than four times that of the 100-inch, but on account of the greater convenience this factor will undoubtedly be increased, as it can probably, in good air, be used without diaphragming down. Four times more light with equal accuracy means that the distance of reach of exploration is increased twice, which means that our visible universe is thus increased 2 x 2 x 2, or the cube, or eight times. If the effective light gathering power be in still greater proportion, as is probable, then we have the universe further increased, but this, of course, is not alone the meaning or object of the instrument.

Every problem which is present with the astronomer today will, with the superb instrument, be more easily reckoned with. Its greater capacity for gathering light makes it easier to use higher magnifications if the image is accurate. It may, therefore, lead to our more complete knowledge of the planets which belong to our own system and assist greatly in the investigation of other heavenly objects which appear in our sky. It may help to solve some of the problems connected with that most interesting neighbor planet of ours, the planet Mars, about which we can form hypotheses but with little chance, thus far, of confirming or denying their validity. The major advantage may be summed up in the terms "greater and more accurate space penetration."

I cannot close this statement without paying a tribute to our laboratory staff, who so far have borne the burden "and the heat of the day," so to speak, in the development which has so far progressed. At the head of this staff is Mr. A. L. Ellis, assistant director of the laboratory, who has been most indefatigable in carrying on the development. They have all done their part, and have done it most enthusiastically and productively. I should like to enumerate them and credit to them their different contributions.

I will close this statement with a quotation from an article which I contributed to the New York *Sun*, on "Electricity During the Nineteenth Century," which was published early in 1901. On the last of the twenty-five pages, the article concludes with this statement:

"The hands of man are strengthened by the control of mighty forces. His electric lines traverse the mountain passes as well as the plains. His electric railway scales the Jungfrau. But he still spends his best effort, and has always done so, in the construction and equipment of his engines of destruction, and now exhausts the mines of the world of valuable metals for ships of war, whose ultimate goal is the bottom of the sea. Perhaps all this is necessary now, and, if so, well. But if a fraction of the vast expenditure entailed were turned to the encouragement of advance in the arts and employments of peace in the coming century, can it be doubted that, at the close, the nineteenth century might come to be regarded, in spite of its achievements, as a rather wasteful, semibarbarous transition period?"

And I might add now, "Has it been any better in the first quarter of the twentieth century?"

TEN UNSOLVED MYSTERIES OF THE STELLAR UNIVERSE

By HARLOW SHAPLEY

(*Read December 6, 1929*)

I AM asked the question, "Why the 200-inch Telescope?" But first I should like to ask *you* a question—why not have some two hundred inch intellects such as those of Aristotle and Benjamin Franklin? Are we not about to see farther than we have vision? To reach farther than we can grasp? As yet that is not the case, I think, and in telling you of some of the unsolved mysteries of the stellar universe I hope to show that we have already visualized the great mysteries, that we have competent detectives on the job, and in one or two of the simpler cases we are nearly ready to ask for an indictment.

The ten major mysteries that I have listed here will be unraveled not alone by the giant telescopes. Possibly one third of the unraveling will be done by the interpreter and by mathematical theory, and another third by the moderate sized telescope that costs hundreds or thousands, instead of millions; but probably a third of the celestial mysteries that perplex us now will yield only to the powerful and penetrating giant telescopes, the greatest of which will be the proposed 200-inch reflector in California of which Professor Thomson has been speaking.

In this outlining of unsolved mysteries we shall begin near at home and travel to the bounds of the universe.

First Mystery: What are the perplexing wobbles of the whirling planet on which we live? The earth does not rotate uniformly and its axis of rotation does not stay put. The poles of the earth wiggle all over the map. Recently Commander Byrd triumphantly dropped the American flag at the South Pole. Did he hit the exact spot? Perhaps, but the pole will soon wander away and give another explorer a

chance. If Commander Byrd missed, the wandering pole may itself find the flag some time.

To be sure these oscillations of the pole are only a hundred feet or so in a year's time. The astronomers here in America can map them accurately by observing the stars that pass overhead. We call the wiggles "latitude variations." Their mystery has been partially solved by the astronomical detectives; two villains are involved—the weight of the yearly fall of snow is one, and the other is a slight deformity of our own planet.

The mystery of the irregularities in the earth's rotation and in the length of the day is unsolved. One of the scientific investigators of this Society, Professor Brown of Yale, believes that they may be caused by changes in the earth's crust. We shall know some day, aided by amateur observers and by the mathematical analyses of their results; aided also by the geologists, for in the attack on many of these mysteries, various sciences must coöperate.

Second Mystery: Where is the original home of the comets? There are probably tens of thousands of these objects—these wandering hordes of meteoric stones in our solar system—but they die off so rapidly under the wear and tear of the sun's intensive radiation and planetary perturbation that in a million years few will be left. Those that exist now must have come into existence more than a billion years after the planets were born. Did we pick up this crop of comets from the wastes of space within the last million years?

The big telescopes will assist in getting the answer—they will follow the faint receding comets to the bounds of the solar system, and help to analyze their light and motions. Big mathematics will also help.

Third Mystery: Why does the sun rotate so rapidly? And the moons of Mars, why so little and so speedy? When a giant passing star, some billions of years ago, wrecked our sun's atmosphere and thus incited the birth of the earth and other planets, was that event an actual collision rather than

merely a close approach? So we now suspect, and the great telescopes may check up on this suspicion through analyses of the atmosphere of the sun, which still shows some effects of the ancient disaster. I have in mind a special telescope for solar research—an instrument of large calibre that will look into the private life of the sun, and help to resolve these puzzles.

The Fourth Mystery is the source of energy that runs the universe. We on the earth live on the bounty of the sun and all our energy, in the last analysis, comes from that star—but how do the sun and other stars get their stores of energy that have been pouring into space for millions of millions of years? It comes not from the burning of their materials, not from the heat of shrinkage, not from electrical or mechanical or chemical sources—such energy stores are all too little. We have a clue to this mystery; the clue is that the stars are transforming matter itself into energy—only a clue as yet, but the big telescopes exploring space and analyzing stars and nebulæ may eventually solve this greatest of mysteries—the source of energy—a fundamental problem of the existence of stars and of life.

Fifth Mystery: Is this universe running down? Are we progressing to the "heat-death" of the universe? It seems so. The stars appear to eat up their own material. They tend to grow little, old, and cold in the course of millions of millions of years. Matter transforms into radiation and disperses throughout the universe. Will radiation sometime or somewhere change back into matter? This is the mystery of mysteries—it is indeed the question of the death of the world. We have only vague clues. Big telescopes and powerful minds may tell us more some day. Now we search, think, and wonder.

Sixth Mystery: Every hour millions of dust-like meteors crash into the earth's atmosphere. Whence do they come? What is their past? What are the obscure secrets they hide concerning the evolution of the planets and the stars and the evolution of the universe in general? We know how to tackle

those meteoric mysteries—just give us research room, and time, and powerful telescopic equipment and we shall contribute a bit to knowledge of the age and development of the world. We compute the ages of stars as something like two hundred trillion years, but what came before the stars? Just give us room and we shall see what we can do.

Seventh Mystery: What are these baffling dwarf stars that are found in the neighborhood of the sun? They are the size of planets but as massive as stars. They have us stumped for the present. Probably there are millions of them but they are so tiny that we can see only the few nearest ones. They appear to be gaseous, like all stars, but unbelievably dense—two or three thousand times as dense as lead. Their further discovery and analysis are problems for big telescopes.

Eighth Mystery: Our galaxy is a great organization containing thousands of millions of stars, our sun being one of this vast number. The nucleus of our galaxy is hopelessly hidden behind dark clouds of matter far out in space, in the direction of Sagittarius. Light cannot get through. We cannot see that remote and mysterious center around which the sun and its neighboring stars rotate with a speed of two hundred miles a second in a period of hundreds of millions of years. The 200-inch telescope, outfitted with powerful accessories, can help in this problem, for gravitational forces operate right through the dark intervening clouds. These forces can be best discerned by measuring the motions of stars which are too faint for existing telescopes. We shall be able to feel the gravitational pull of this great galactic nucleus, though we cannot see the stars that are pulling.

Ninth Mystery: Why do the other galaxies of stars, that lie far outside our own, run away from us with terrific speeds? The great spiral nebulæ, now known to be island universes, composed of millions of stars, all appear to shun us—to fly in all directions. Is this real motion? Or is it a relativity effect? Or some deeply significant but as yet undiscovered property of space and time? Only the greatest of tele-

scopes can contribute facts for the solution of this remarkable situation.

And finally, the Tenth and perhaps most hopeless Mystery: Is there a limit to the explorable universe, a limit to creation? Or do the galaxies extend on and on into impenetrable depths of space? Already the astronomers are measuring galaxies that lie a billion trillion miles away, so far away indeed that light takes more than a million centuries to reach the earth. The great telescope now under construction—the 200-inch reflector—should go further and tell us either that we now approach comprehension of the whole galaxy of galaxies, or that we must have still greater telescopes and deeper thoughts before we can hope to grasp and understand the total of material creation.

ANTARCTICA

By ISAIAH BOWMAN

(Read February 7, 1930)

An invitation from the American Philosophical Society is a command. As a member of this oldest learned society in the United States it is with great pleasure and a sense of high obligation that I address you upon invitation of our President. While the new hall will provide safer quarters for the future you will permit me to express heightened pleasure in speaking in the old hall built 141 years ago with the money raised by Benjamin Franklin and his fellow-members and business friends, and thus to meet with you among these priceless scientific and historical possessions contributed in the past by an extraordinarily distinguished body of men that includes the names of George Washington, Thomas Jefferson, Charles Darwin, Theodore Roosevelt, Andrew Carnegie, and Woodrow Wilson.

Among a membership pledged to "the mutual communication of their discoveries," as the original charter of 1780 puts it, there cannot fail to be great interest in the Antarctic Continent, the seventh and last to be explored. That interest is heightened no doubt by the defiance with which Antarctic elements obstruct the inquisitive spirit of man. In the magnitude of the forces of ice-cap and wind and encircling water and that "sullen barrier" of pack-ice that guards the outer seas of Antarctica, it is as if we had a transplantation of some great cosmic agency that has wrought a continent of incredible inhospitality at the South Pole as a symbol of outer worlds of mystery beyond the reach of man.

This is not fancy merely, but rather the result of sober scientific reasoning. Reflecting on the great contrast between the north and south polar regions, the one a hollow, the other a hump, Chamberlin speculated on the possibility

that we have in these and other lineaments of our earth actual "birth marks," as we may call them. He saw in the assembly of materials of which the earth is composed traces of the original bolt of matter shot out from the sun to make the infant earth. The core was built up of heavier material at the end originally toward the sun and of lighter material at the end away from the sun. The heavier Antarctic end was of course still further modified in the course of time while the lighter materials eventually were shaped into the ring of land that lies chiefly in the northern hemisphere. This might be called the "ring of life" because it made that wide belt of dry land upon which the higher types of life emerged that reached their climax in civilized mankind.

I can imagine Franklin asking Chamberlin why it is that science is so greatly interested in the Antarctic. And I can see Chamberlin pointing to the two unlike ends of the earth. "Here," he would say, "is the north-polar hollow which an echo depth-sounding made in 1927 by Captain Sir Hubert Wilkins, at a point 500 miles northwest of Point Barrow, Alaska, showed to be over 17,000 feet deep; while at the south-polar end you see plainly a huge protrusion, the Antarctic Continent, bearing mountain ranges over 10,000 feet high which rise from a platform itself 15,000 feet above a ring of encircling ocean deeps."

The life of the two ends of the earth is as unlike as the topography. The ring of land about the Arctic Basin has yielded fossils in comparative abundance, and, together with the abundance and variety of the plants and animals of today, makes possible rather definite conclusions about the migration of species from continent to continent in the subarctic zone. No such definite conclusions can be formed in the Antarctic. Exploration must go much farther before the Antarctic chapter can be written. Indeed, Antarctica is still so great a mystery that were a fossil marsupial to be discovered there the event would excite scientists probably as much as direct radio communication with Mars.

Were Benjamin Franklin, the founder of the American

Philosophical Society, to hear such a conclusion he would never ask "What is the good of exploration in the Antarctic?" In fact, he could not have raised doubts about the value of inquisitiveness in any form under any circumstances. When

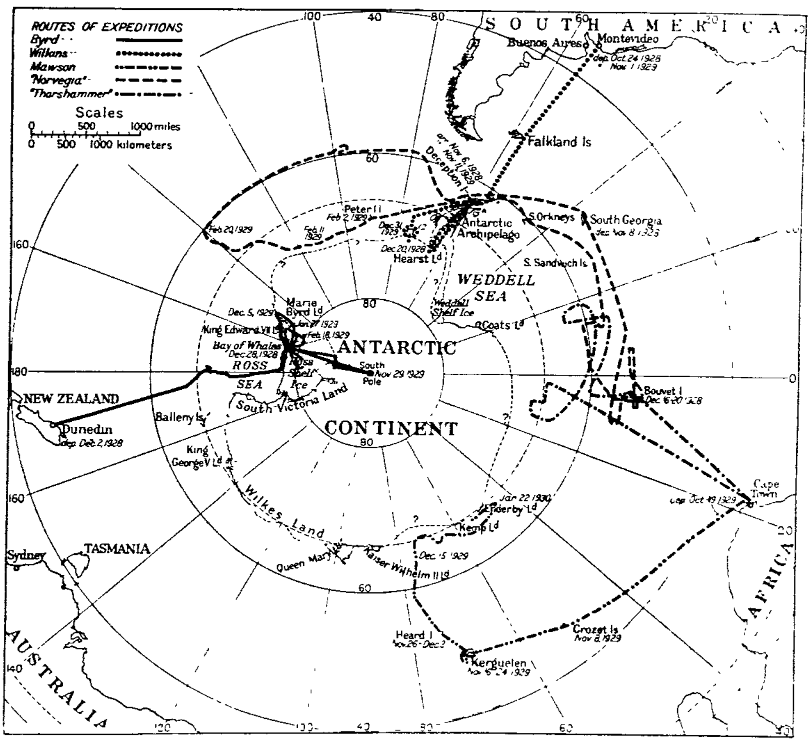

FIG. 1.—Map of the Antarctic showing the routes and locations of the four expeditions in the field at the time the accompanying address was delivered. For references to the latest maps, books, and articles on the Antarctic see the note on sources at the end of this article.

a scoffer, knowing of Franklin's interest in balloons, asked "What good are they?" he answered, "Of what good is a new-born babe?" and then went on to contribute money to aeronautical experiments, never missing a balloon ascension if he could manage to attend. The man who sent his kite into the clouds to ask questions of the lightning was one of the founders of what we might call the "brotherhood of curiosity." Its

members want to know nothing less than all that there is to know about this amazing universe.

It is that driving curiosity about scientific problems still unsolved that has moved Byrd and Wilkins and Mawson to match Antarctic defiance with calculating skill and courage. It was curiosity, paired with technical training, that led to the development of radio communication, and, as a result of that curiosity, it is possible to stand here and be heard by Byrd, while at the same time Wilkins and Mawson, thousands of miles away, may hear the same message.

Admiral Byrd, I have been asked by President Dercum to send you a personal message from the American Philosophical Society. He has asked me to say how deeply moved we all are by the reports of your expedition, how interested in its success, how anxious over its safe return. You have gained results not of scientific worth merely but of deep interest to all mankind. President Dercum has asked me to invite you to address the Society upon your return that the officers and members may hear from you directly. What I have to say in the address that follows this brief radio introduction will be chiefly through the medium of twenty or thirty maps that summarize the results of your expedition and those of Wilkins and Mawson. You have no conception how personal to each of us your affairs have become. Morning, noon, and night we wonder about the state of the ice—will it open in time, can the *City of New York* get through? When that ship was about to sail from New Zealand a few weeks ago there must have been many who recalled Arthur Colton's lines:

> "And one ship in port today
> On the morrow
> Southward bound will far away
> The swift sea furrow;
> Whom the loud Antarctic waits
> And frozen citadels with creaking gates."

Our invitation is no less warm to Sir Hubert Wilkins and Sir Douglas Mawson. Through special arrangements this

message goes directly to you both and as a personal friend of both I want to say with what deep admiration I follow your fortunes and wish your gallant enterprises success. I know that Admiral Byrd shares and often expresses equal admiration. And when the time comes for you to communicate your results to this learned society I feel sure that the Admiral will be here to lead the discussion. It would gratify this assembly if within the next few days we might have acceptances to these three invitations to meet in this old hall and hear "the mutual communication of your discoveries."

All three of you have brought the Antarctic to the clubs, the schoolrooms, the family firesides of millions the world around. Would Gould succeed in finding the coal layer he was after? The question got to be a poignant one! Would Wilkins find the base he sought? Where would the ice halt Mawson? We have found ourselves involved in an "Antarctic Series" with this curious condition that everybody is betting on the success of all the teams, the three we have mentioned and also the Norwegians, particularly our old friends, Major Isachsen and Captain Riiser-Larsen.

This wide interest we owe not only to yourselves but to the newspapers; and the time has come when science as well as the general public should acknowledge its indebtedness to the press. Without the assistance of the newspapers the well-equipped expeditions of recent years could not have been undertaken. However efficient airplanes may be, they are expensive things. Moreover we are far more interested in an expedition from which we can have almost daily radio reports than we are in one that vanishes for several years, returns with news that blazes for a week, and then drops into the gulf of forgetfulness. But for the extraordinary support of the *New York Times*, which includes a triumph in radio communication, Byrd's expedition would have been impossible and public interest in it short-lived. The *Times* has contributed in this way not merely to public interest, for which it has no doubt already gained some measure of reward; it has gone far beyond this and made a contribution to science as substantial as many

a scientific institution. In the same spirit the *Hearst Newspapers* have supported Wilkins, helping him with his equipment, maintaining radio communication, and assisting in the publication of his results. This group is also supporting the expedition of Sir Douglas Mawson. It is but fair that we take a moment from the consideration of the scientific results of three major Antarctic expeditions to praise these two newspaper agencies for the public-spirited way in which they have gone beyond commercial objectives and made direct contributions to scientific discovery and public education.

But if science is to reap the full benefit of the opportunities thus thrust into its hands the scientific side of the story should be as well presented as the popular side. To this end the American Geographical Society of New York has for some years attempted the constructive task of publishing a series of books and maps of direct value to the explorer in the field as well as to scientists and laymen at home. It is extraordinary that this has not been done in the past quarter century. The explorer in the field has had to turn from one map to another. In a series of Arctic and Antarctic maps on different scales the Society has brought together original surveys and meteorological and magnetic data with the result that field operations might be planned upon a single large base map. Our bathymetric map of the Antarctic is the only one of its kind in existence that shows the present status of our knowledge of Antarctic submarine relief on an adequate scale.

At the same time the Society prepared two volumes which expedition leaders have described as an explorer's bible—*Problems of Polar Research* by thirty contributors, which include practically all the leading polar explorers of today, and the *Geography of the Polar Regions*, which describes the Arctic and Antarctic region by region. These books and maps pool the knowledge of practically all living polar explorers. They have been put into the hands of trained men on each of the present Antarctic expeditions and have led to the search for particular things, not just anything. Modern large-scale exploration, to be worth its cost, must be scientific, not merely a blind and adventurous wandering into the unknown.

If we look at the Antarctic today through the eyes of those who have specialized in its interpretation and who are searching for facts about it at this very moment we shall see its real importance. While there are over four hundred species of flowering plants in the Arctic, some of them luxuriant, there are but two in the Antarctic and these have a precarious hold at the extreme limit of their range. They are both found on the western side of "Graham Land." One is a grass, the other represents a family of herbs. Both are dwarfed. There are no ferns in the Antarctic. Only local tundras (of mosses and lichens) up to a half acre or more are known. The short summer and the remarkably low temperature (no month has a mean temperature above freezing) is responsible for this poverty of plant life. By contrast the Arctic summer has a mean temperature above freezing during the warmest part of it and the ground may thaw a few inches in a very few days. From these few indications you see that the present life of the Antarctic is too meager to furnish a basis for wide-ranging speculation concerning past migrations between Africa and South America or between Australia and Africa. The search for fossils is therefore keener than ever because it is chiefly through *them* that explanations are sought of present distributions of life by migration across the Antarctic region. These are not merely the abstractions of the specialist. The layman can get the sense of it. Did it ever occur to you how remarkable it was a year or more ago to have a whole column in a New York newspaper devoted to the finding of pre-Cambrian fossils by Sir Edgeworth David of the University of Sydney, Australia?

It is in the rocks of Antarctica that we find the greatest realm of wonders. In earlier periods the life was far more abundant. For instance, the whole earth, Arctic and Antarctic included, enjoyed a mild climate in what geologists call the Jurassic period. At that time there lived in West Antarctica the Sequoia, the Araucaria, and the Beech. We know this through fossils brought back by explorers. The former range of animals as well as plants is determined in the same

way. The value of such records is so great that the search for fossil-bearing rocks in Antarctica has now become one of the major sports of science. That is why Professor Gould attached so much importance to the carbonaceous material which he found in the sandstone from Mt. Fridtjof Nansen. Even the contents of the stomachs of seals and penguins have been searched for rock specimens and thus much valuable information has been found about coastlines where no exposed rock exists. The dredge has brought up from the bottom of the Weddell Sea, from a depth of 10,000 feet, fragments of Cambrian rocks; and the ice tongues that pour out through the mountain passageways about the Ross depression have borne from distant points limestone fragments of the same age that throw light upon past conditions.

It is the higher forms of life however that interest us most for here we are closer to the background of man himself. Where we now have a highly specialized group of birds of which three are exclusively Antarctic species—the skua, the Adélie penguin, and the emperor penguin—there was once a greater variety. If we include fossil penguins of wider range the number of extinct species already known rises to twelve. It has been suggested that the diminished number of species and their higher specialization today were brought about by the advancing ice in a period of glaciation even more severe than the present one, for the ice at the present time has withdrawn from the wider limits it once claimed. If Antarctica today seems almost buried in ice, we can only say that in a still earlier period it was *overwhelmed*.

These are problems of geological history but there are problems of present-day life no less interesting. Many people must wonder why it takes so long to plan a polar expedition. The matter would be simple if the explorer faced perfectly definite conditions of climate and ice. Both pack-ice and weather are never the same for two seasons in succession. In spite of all the studies that have been made upon the pack-ice, and all the temporary passages that have been traversed, we still have very little knowledge about its be-

havior over substantial periods of time. It presents new problems every year. Admiral Byrd's low-powered ships were able to get through it without special difficulties when the party was deposited at Little America last year, though there were risks and trials enough. This year it is exceptionally heavy and presents a barrier three hundred miles across. Ice of that sort has to be "humored," as the sailor puts it. All eyes are upon the month of February. It is just now or at most very soon that there is the best chance of penetrating the pack and coming out again quickly. By the middle of March heavy freezing begins again. In varying width, and with breaks here and there, the pack-ice almost encircles the Antarctic Continent, in some places standing offshore for some distance, in others continuous with the ice foot of the land or the discharging ice-cap. Almost everywhere it has to be reckoned with. Mawson faces the problem near Enderby Land today. A few weeks ago Wilkins was held off from the land 150 miles north of Charcot Island. Riiser-Larsen, operating near Enderby Land, was able to fly to the edge of the shore ice and make his way to land on skiis, taking possession of a bit of land in the name of Norway.

The American Philosophical Society has as its main purpose "the promotion of *useful* knowledge." What have these scientific questions to do with practical matters? In spite of all the pride of achievement in so-called "pure" science, the scientist still enjoys the excitement of discovering a practical application of a principle. It was said of Franklin's lightning rod that through it progress had been made which all could utilize as well as see and understand. By it "science had reduced the realms of religion and annexed those provinces which had hitherto belonged to Faith and Prayer" (Bernard Faë).

Of course every one knows that the Antarctic is a great source of whale oil (used principally in the making of soap), and we know how extensive that industry has become. Most fortunate it is for the cause of scientific discovery that it pays to catch in Antarctic waters, even if the whalers have been

inclined to keep their discoveries of new whaling grounds and the behavior and location of the pack-ice hidden as a commercial secret. They have made whale oil pay for knowledge, combining real exploration (and great hospitality to other explorers) with economic exploitation. This is quite in the tradition of polar exploration. Old whaling captains will still tell you of that knight errant of Arctic whalers, William Scoresby, commander on thirty voyages, who chose the coast of Greenland in preference to Spitsbergen not only because he sought whales but because he had the hope "of making researches on a coast that was almost entirely unknown." He prefaces his surveys with the assurance that "An excellent cargo of whales . . . was obtained," but geography records an indebtedness greater than that of the whale-oil industry in the name "Scoresby Sound" permanently affixed to the map of Greenland.

Whaling itself cannot be more than a minor industry at best. Are there larger utilities to be sought in Antarctica? If we ask the question of agriculture in the southern hemisphere the answer is in the affirmative. It would pay handsomely in crops and cattle and security of life if meteorological stations were set up on the borders of the Antarctic and on the island groups that girdle it. If we knew the habit of the "spells" of Antarctic weather there is little doubt that we should be able to find a connection between them and the rainfall and drought periods in the cereal and pastoral lands of Australia, South Africa, and Argentina. It is under the impulse of this idea that Captain Sir Hubert Wilkins has carried on his explorations in the Antarctic Archipelago for two seasons. He is not down there just for fun; he is searching for suitable bases for meteorological stations to be established by international coöperation. With a ring of such stations about the Antarctic, and with daily radio reports from them, on the weather, it would be possible to draw charts that would trace the effects of cyclones and anticyclones as they move forward from breeding places out over the southern ocean.

Some men shudder at the implications of this consuming

curiosity of man, for is it not written, say they, that "It is not for you to know the times or the seasons, which the Father hath put in his own power." Emerson always drew wide limits to a question but even he remarked "Our own orbit is all our task, and we need not assist the administration of the universe." In trying to understand Antarctic weather, science is not trying to run the terrestrial weather machine. We are not asking the earth to be kind; we only ask her *to announce what she intends to do!* To forecast seasons of drought in the lands of the southern hemisphere would be a practical achievement of the highest order, and no less important would it be to forecast seasons of exceptional rain. We have in Australia, Argentina, and South Africa great areas of marginal lands where for several years on end it may be too dry to maintain flocks and herds and crops of normal extent. Even in years of sufficient rain the farmer needs notice of the event to enable him to take advantage of nature's bounty.

It is not putting the case too strongly to say that the practical benefits of meteorological studies in the Antarctic through the medium of a chain of weather stations outweighs all other Antarctic interests put together. We have had international coöperation for such purposes in both the Arctic and the Antarctic, the former nearly fifty years ago, the latter twenty to twenty-five years ago. But these were periods when we knew far less of the connection between the weather of high latitudes and that of the more habitable lands in which most of us dwell. From both the scientific and the practical standpoints it is imperative that such stations be established at the earliest possible moment if we are fully to occupy those pioneer lands of three continents where the risk of loss still holds back settlement or puts a cruel strain upon it.

However briefly I have sketched the scientific problems of polar lands I hope, ladies and gentlemen, that you see in polar exploration not adventure merely but serious business pursued for definite and important reasons by those rare men who combine physical strength and courage with the scientific imagination.

(End of radio address)

Section of Paper on Antarctica to the American Philosophical Society in Extension of Previous Remarks by Radio

(Illustrated with wall maps and lantern slides)

Exploration is somewhat like a military campaign. This is especially true in a territory so formidable as the Antarctic in size and in climatic severity. The first thing is to make a reconnoitering attack to get the outlines of the situation. It may seem like a very simple object, that of going out to map the border of the Antarctic Continent, but until this is done no one may safely conduct a long campaign in the interior. It was wise planning for Byrd to base his flights on the Bay of Whales at the edge of the Ross Shelf Ice which placed him more closely to the Pole than he could get elsewhere by ship and where he met conditions fairly well known from previous explorations. In a similar way Wilkins based his operations on Deception Island, where he could depend upon the help of the whalers and from which he could explore that long finger of Antarctica which stretches up toward South America. For two seasons he has been searching for a base as reliable as Little America from which he could take off with a full load for the more than two thousand-mile journey to the Ross Sea.

In geography we attach very great importance to this first stage of exploration. When the outlines of the continent have been mapped and the position of the offshore islands determined; when we have photographs of the coastal features, and know the habits of the pack-ice that all but surrounds the continent; when we know the location and width of the shelf ice that fringes the shore in places; when the habit of the winds is better known—then the more intensive studies at critically selected points will be vastly more profitable.

Right here we have a most important fact, that the pack-ice is never the same two seasons in succession because the temperature varies, the force of the wind is variable from year to year, and there are corresponding effects upon the currents. Very much useful work remains to be done upon the pack-ice

itself. Until we can establish its habits we can not plan wisely with respect to the critical points of attack in the course of future expeditions to the still unknown sectors of Antarctica.

We take airplane and radio so much as a matter of course that it is only occasionally that we realize what they mean in speeding up the reconnoitering stage of polar exploration. Until November 16, 1928, no airplane had been flown in the Antarctic. On that day Wilkins made his first flight from Deception Island. Compasses and other instruments have been immeasurably improved. Sounding devices are now in course of construction for operation from a speeding airship. Canned heat as well as food can be carried. There need be no fear of starvation or scurvy. Byrd timed both his north and south polar flights, to correspond with short periods of clear weather. A vast amount of work can be done with present day instruments in *a single spell of good weather.* Wilkins' flight over "Graham Land" was likewise timed by the weather. On his great flight from Alaska to Spitsbergen, he spotted a storm far ahead, flew around it, and thus maintained his visibility and made certain of his position. The radio enables the time to be obtained precisely and has become an important element in position finding besides vastly increasing the security of the polar explorer. He can be in constant communication with sources of information at home. On several occasions the American Geographical Society has been requested to supply, by radio, technical information concerning maps and soundings for the Byrd expedition, as it did for Wilkins before his Barrow-Spitsbergen flight. As new conditions arise the explorer in the field can now call upon the scientist at home for advice and assistance. He can command resources as quickly as if he had the advantages of a post office or a telegraph station at hand. The possibilities of such collaboration in the future are very great, since the explorer in the field can obtain information and advice which were totally inaccessible to his predecessors.

Unquestionably the most important information in the reconnaissance work that represents the first stage of explo-

ration is gathered by the camera operated from the airplane. By this means there have been outlined in two seasons by Wilkins practically the whole of the hitherto unknown portion of the Antarctic Archipelago and the coast of Hearst Land for several hundred miles. Byrd has photographed a stretch between Little America and the Pole and has added territory east and northeast of Little America which includes a newly discovered mountain range, as well as Marie Byrd Land and the Rockefeller Mountains discovered last year. When these photographs are built into a mosaic and reduced to a map we shall have substantially a topographic base map from which later operations may be planned into the still undiscovered country towards the east.

Every subsequent explorer will have his way made easier by such precise information. In addition, we shall be able to see the relation of mountain to mountain and build up those hypotheses which will lead to better planning in the search for critical information that still remains to be found before we can complete our picture of the form and build of the Antarctic Continent. This is the way of science. A new discovery is not an end to anything. It is a step in an unhalting advance that began when speech and fire and the spear and the floating log were no longer mysteries to awakening man.

Broadly speaking, there are two areas of greatest interest from the standpoint of reconnaissance work. The first is the tracing of the depressions of the Ross Sea and the Weddell Sea to their heads in order to determine if there is a salt-water connection right across Antarctica, thus making a great island of the territory between Little America and Hearst Land, or possibly a group of islands. Both Byrd and Wilkins hoped to carry their flights inland far enough to determine this point, which was among their announced objectives. But neither has succeeded in doing more than continue the probability that these two depressions run farther inland than we had been led to expect. Most significant is the discovery of Gould that the Ross Shelf Ice extends at little elevation above sea-level, past the 150th meridian, and swings eastward along the front of

the Queen Maud Range farther than we had been led to expect from the reports of Amundsen, whose Carmen Land now drops out of the map. Even if a salt-water connection were found to exist it is altogether probable from the slight elevations already determined at the heads of these depressions and from the soundings on the border that the connection is ice-filled throughout. And if an archipelago exists to the northward of this depression the separate land units are probably "welded" together, to use Mawson's term, into a single unit continuous with the larger part of the continent bisected by the 70th meridian east of Greenwich.

Two great objectives are the determination of the position of the coastline between Enderby Land and Coats Land and from Hearst Land to King Edward VII Land. The flights of Byrd and Wilkins have cut off 600 miles of hitherto unknown coast along the southern Pacific Ocean, reducing to 1,600 miles the unexplored section. The Coats Land-Enderby Land coast is of equal length and is succeeded by another stretch half as long east of the 50th meridian.

In the field season 1928–29, after establishing himself at his camp at Little America, Byrd undertook a flight to the northeast as far as Alexandra Mountains, from which he flew south to discover a new range which he called the Rockefeller Mountains. On a second flight past the southern end of the Rockefeller Mountains he discovered high land beyond the 150th meridian west which he named Marie Byrd Land. Fortunately, it was possible to make a second flight the same day with the aerial mapping camera and thus to provide the basis for a detailed map; and later on Dr. Laurence Gould, the geologist of the expedition, flew to the Rockefeller Mountains and collected rock specimens.

In the season just closing, Admiral Byrd has been able to make a number of flights of which two are of outstanding importance. A first was made on November 28 and 29, 1929, when again the aerial mapping camera was employed on an 800-mile flight to the Pole. The round trip of 1,600 miles was made in nineteen hours, including a stop of one hour at a

refueling base. Of importance to the success of the flight was the radio report on the weather as far as the foot of the mountains over 400 miles from Little America, made by Dr. Gould's geological party. In return Byrd was able to help the geological party by dropping a package of photographs taken on a previous base-laying flight, thus enabling Gould the better to plan his advance to his distant goal, Mt. Fridtjof Nansen and the Queen Maud Range. Byrd's second flight was intended to extend his discoveries toward the east of Little America while at the same time he established an independent communication with the sea-coast outside of the Ross Dependency. In this manner he was able to set up the basis of an independent claim on the part of the United States to newly discovered territory tied directly to the coast.

It was his good fortune to discover a salt-water entrant on this flight which seems to cut off King Edward VII Land from a new mountain range extending along the 147th meridian west, to trace 100 miles of new sea-coast, and to connect the new mountain range with Marie Byrd Land in the interior, thus providing the framework for a physiographic description of an entirely new group of topographic features. At the same time the trend of the newly discovered coast throws light upon the hitherto baffling question as to why there should be such a great concentration of ice in the Ross Sea and in the region to the northeast. The whaling operations of the Norwegians have shown the pack-ice thereabouts to extend far to the north as if it were held back by land still untraced or by groups of islands that might serve equally well to confine the ice in the direction of the Ross Sea.

The sledge party under Dr. Gould which had Mt. Fridtjof Nansen as its objective was able to explore new territory and at the same time gather rock specimens from localities so close to the Pole that both rocks and fossils have exceptional value in determining the probability or improbability of a connection between the Queen Maud Range and the Antarctic Archipelago. New hypotheses of origin may thus be set up that will help direct future explorers in their search for additional in-

formation. From 6,500 feet elevation on the flank of Mt. Fridtjof Nansen, Gould found a sandstone that includes a layer of carbonaceous material, thus extending to this distant interior point the so-called Beacon sandstone found at other points in the long stretch of South Victoria Land on the west side of the Ross Sea, and making it more probable that the south-polar plateau, east of the 160th meridian east, is underlain by a vast coalfield.

Wilkins' great flight from Deception Island, 600 miles south, on a meridional course, is one of the great feats of exploration by airplane in the Antarctic. In five and a half hours of outward flying he transformed our whole conception of so-called "Graham Land," proving that it was broken into separate units and that as a whole it was separated from the main body of the Antarctic Continent by scattered islands and a water passage filled with ice. The name "Graham Land" thus drops off the map and the whole group of islands may more properly be designated the Antarctic Archipelago. Through his photographs from the air and the positions indicated on his route it was possible to construct the outlines of the eastern coasts of the main islands composing the archipelago as shown in the July 1929 number of the *Geographical Review* and as reproduced upon the map accompanying that issue.

It was a most startling and important discovery that Sir Hubert had made and it is a great satisfaction to record that in the present field season he was able by a short flight from a base on the western border of the archipelago to check his determinations of last year and show the substantial accuracy of the positions indicated upon the map that originally appeared in the *Geographical Review.* He made a four-hour flight on December 31, 1929, which enabled him to trace what appears to be the mainland of the Antarctic Continent westward just south of the 70th parallel as far as the 80th meridian. He took off from the water beside the *William Scoresby* at 67° 47′ south and 75° 21′ west. On this flight he proved the insular character of Charcot Island, and photo-

graphing the island and the mainland shore and observing the trend of the shore, east and west, he was able to connect with his discoveries of last year and establish the relative positions and relationships of the Antarctic Archipelago and mainland in a manner invaluable to future exploration and very gratifying to the cartographer.

Upon the other side of Antarctica there is an expedition of which we shall hear much more before the end of another year. It is conducted by Sir Douglas Mawson and sponsored by the governments of Great Britain, Australia, and New Zealand. As on his previous expedition of 1911–14, Mawson proposes to explore that part of the Antarctic border that faces Australia, as well as the section between 50° and 80° east. He hopes to make studies on the continental shelf that will be primarily oceanographic in character. His ship, Scott's old *Discovery*, is equipped with modern apparatus for oceanographic work. He expects to take a large number of soundings on the continental shelf; determine the outer border of that shelf, which is, in fact, the outer border of the continent itself; investigate the physical and biological features of the water overlying the continental shelf; and make such studies of sea organisms and sea floor as will throw light upon the origin of the continent as well as the distribution of life in a manner that may be useful to the whaling interests. He has an echo depth-sounding machine as well a drum and cable for making soundings of the conventional type. He will secure bottom and water samples and, by means of a small scouting plane, has planned flights from the ship over the ice fringe in order to determine, if possible, the outlines of the coast hitherto unseen over much of the section that he expects to explore.

Mawson has already done important work. He has gone by way of Cape Town, Possession Island (one of the Crozet group), and thence south to a point in the ice-pack about on the 75th meridian east, being held off from the land by heavy pack-ice. The soundings becoming shallower, a flight was made in the scout plane on December 31 from 66° 11′ south and 65° 10′ east, and on this flight, beyond a 40-mile belt of

unbroken ice and 10 miles more of coastal water, there were sighted what appeared to be low, hilly ice-covered tracts of land.

Mawson's concentration upon the continental shelf is in line with the most advanced scientific thought concerning the meaning of this shelf, and the life found in the waters upon it, in the recent up and down movements of the Antarctic Continent and the heavier glaciation that preceded the present stage of ice retreat. Though the land life is poor, the shallow water life about the rim of the Antarctic is amazingly rich. Owing to the upwelling of the deeper waters near the shore, as the strong winds brush the surface waters away from the continent, there is brought from below the deeper oceanic waters rich in nitrogen. The coastal waters also contain an abundance of silica owing to the low temperature and to the large quantity of rock waste swept down by individual glaciers as well as the Antarctic ice cap. There are 0.50 part per million of nitrogen in Antarctic waters in contrast to the 0.15 + part per million in the North Atlantic and 0.10 in tropical oceanic waters.

The high nitrogen content of Antarctic waters makes them an ideal home for those immense quantities of diatoms that furnish the base for higher forms of life in succession. This is the key to that immense development of seals, penguins, and whales that excite our curiosity by their appearance in waters adjacent to the coldest, most desolate, and most terribly windswept land mass in the world, the "home of the blizzard" as Mawson called it. Mawson saw 16½ acres of penguins in Macquarie Island, half way between New Zealand and Antarctica, and it is estimated that a million penguins were observed in one rookery in the South Orkneys, in latitude 60° on the northern border of Weddell Sea.

No account of Antarctic exploration would be complete without reference to the scientific work carried out, especially in the last few years, by Norwegian whaling interests. Wishing to free themselves from the obligation to pay a license fee to the British Government, the Norwegians have steadily

developed larger ships capable of maintaining themselves for a season at sea in areas outside those under British jurisdiction. To help them toward an independent status they have annexed Bouvet Island southwest of Cape Town, about on the 55th parallel, and Peter I Island, which lies at the edge of the pack-ice on the 90th meridian west of Greenwich, or 10° west of the western border of the Falkland Islands Dependencies. During the last three Antarctic summer seasons four expeditions have been sent out and in the present season, 1929–30, three Norwegian vessels are in the Antarctic engaged in scientific exploration. Both Major Gunnar Isachsen and Captain Riiser-Larsen are now in the Antarctic in vessels equipped for exploration, and reports have been received of operations between Coats Land and Enderby Land, where fast land has been discovered and "possessed" for Norway. In this they were helped by scout planes by means of which they were able to fly from their ship 100 miles to open water from the edge of which they traveled by skiis, raising the Norwegian flag on the shore. The whalers have also skirted the pack-ice west of the Antarctic Archipelago right around to the 140th meridian west, thus making many new determinations of the position of the ice that will be of great value to science when available in published form.

The physiographic interpretation of the land forms of Antarctica only now reaches the scientific stage. The work of Wright and Priestly on Antarctic ice has laid the ground work for a better understanding of the land forms we may expect as a result of the action of such diverse agencies. Wilkins' photographs of the eastern aspect of the Antarctic Archipelago reveal an apparent correspondence between the agencies that have shaped this part of the island chain and those already recognized through the work of the French and Belgian Antarctic Expeditions of twenty-five years ago.

The land is clearly in a stage of relatively recent uplift and fragmentation, following a long period of stability in which a peneplaned surface was formed. Cretaceous and early Tertiary rocks were folded and intruded with igneous rock before

the peneplanation took place so that we may say that peneplanation is of rather recent occurrence and that erosion in the present stage of uplift has not gone far enough to destroy clearly recognizable fragments of the former more nearly continuous surface.

The remarkable thing about these signs of a former low-featured landscape is their correspondence with features of similar age in South America whither the Antarctic Archipelago points and with which it is all but connected by a bridge of islands and relatively shallow banks and ridges that sweep in a great arc by way of the South Orkneys and South Sandwich Islands. Throughout the length of the Andes there is a clearly traceable uniformity of topographic types—mountains of Alpine height and ruggedness, many of them snow-clad, rising above high plateaus (whose surfaces bevel the disordered rock structures of earlier mountain-building periods), steep-walled, broad, high-level valleys of mature development, and finally canyons cut deep into the plateaus. The whole assemblage of forms is so well marked that the history of the landscape is clear in its broad outlines at least. The plateaus of erosion represent a long period of crustal stability in which all but the Alpine heights were reduced to a surface of low gradient with wide open valleys. Then came uplift and the development of a new set of valleys that represent a second long period of stability, and finally a truly remarkable uplift of 5,000 to 10,000 feet at least (in the Central Andes) which brought the land to approximately its present position. (For the sake of brevity no mention is here made of the complications of relief brought about by vulcanism. For details see my "Andes of Southern Peru.")

These features and the history they represent are repeated, with variations, from Colombia to Patagonia. They seem to be repeated in broad outlines in the part of Antarctica nearest to Patagonia. The ice and late volcanic outpourings have introduced strong variants in the late history of portions of the land surface. The importance of this correspondence of physical history over so wide a terrain may prove of value in

studying the fossil records, since the topography takes up the story and bears the impress of events that followed the close of the sedimentary record. It suggests that the sedimentary period itself may have had a similar uniformity over at least southern South America and Antarctica, thus providing the conditions for the wide distribution and migration of life forms now separated by deep seas that cover the foundered blocks of an ancient land bridge.

The foundering of these intervening crustal blocks is itself a problem of extraordinary interest in geophysics. The topographic forms of Antarctica in the Atlantic Quadrant seem to continue the story of western South America where the entire coast represents an eroded and modified fault scarp or series of scarps. The Pacific crustal block, dropped in places to abyssal depth, was sundered from present land continuations in late geological time. What little we know of the Antarctic Archipelago suggests a similar breakdown or fragmentation of the Antarctic border of the southern Pacific. How and when it occurred, and what its wider bearings and implications may be in the geomorphology of the southern hemisphere, only future research will reveal.

NOTE ON SOURCES

The preceding paper is based in part upon recent publications of the American Geographical Society of New York. Among the books and maps on the geography of the polar regions which the Society has produced special reference should be made to the following:

BOOKS AND ARTICLES

1. Problems of Polar Research
   By thirty-one authors. In this volume thirty-one of the outstanding men in polar exploration (including Nansen, Byrd, Wilkins, Mawson, Rasmussen, and Stefansson) have contributed ideas as to the methods and objects of future work by all the known means of research.
2. The Geography of the Polar Regions
   By Otto Nordenskjöld and Ludwig Mecking. The most complete regional reference book in English on polar geography. The first part is a characterization of polar nature; the

second is a description of the Arctic and the Antarctic, region by region.

3. BRIEF HISTORY OF POLAR EXPLORATION SINCE THE INTRODUCTION OF FLYING
   By W. L. G. Joerg. A summary of all of the aerial surveys and explorations in both Arctic and Antarctic territory up to the moment of printing, *i.e.*, December, 1929.
4. ARTICLES IN THE GEOGRAPHICAL REVIEW
   In 1917 *The Geographical Review* began the publication of a series of articles and maps on polar geography that has come to include over fifty separate items. They relate to the leading expeditions and their work and represent in many instances the definitive scientific results. A full list of the Society's publications in this field is found in Special Publication No. 11 (see note below).

MAPS

5. MAP OF THE ANTARCTIC, 1 : 4,000,000
   A four-sheet map in two colors. Shows known elevations of the land, including Scott's and Amundsen's lines of march to the South Pole, and ocean soundings and relief by contours. There are also shown lines of equal magnetic variation and wind roses for stations at which consecutive weather records were kept.
6. MAP OF THE ANTARCTIC, 1 : 12,500,000
   This map consists of (1) a main map in two colors on the scale of 1 : 12,500,000, showing the Antarctic in relation to the southern continents; (2) an inset in three colors on the scale of 1 : 2,500,000, showing the Antarctic Archipelago, the route of Captain Sir Hubert Wilkins from Deception Island to Hearst Land and return on his flight of December 20, 1928, and the newly discovered features; and (3) a series of ten photographs taken by Sir Hubert Wilkins on that flight.
7. PHYSICAL MAP OF THE ARCTIC, 1 : 20,000,000
   A physical map, in color, showing primarily the relief of the land (in layer tints combined with hachures). It embraces a circular main map of the area from the pole to latitude 58° N. and insets, on larger scales, of the northern outlet of Baffin Bay, of southwestern, northernmost, and northeastern Greenland, Jan Mayen, Bear Island, the Ice Fiord region of Spitsbergen, Spitsbergen (Svalbard), Franz Josef Land, Novaya Zemlya, and Bering Strait.

8. Bathymetric Map of the Arctic Basin, 1 : 20,000,000
A revised edition (to 1927) and an entirely new drawing of Dr. Fridtjof Nansen's standard map, stressing the contrast between the continental shelf and the deep basins of the central Arctic Sea and the "Norwegian Sea." Bathymetry is shown in three tints of buff for the continental shelf and four tints of blue for the deep sea and the majority of critical soundings are indicated. In addition the general aspect of the relief of the land is shown in five layer tints.
9. Bathymetric Map of the Antarctic, 1 : 20,000,000
A bathymetric map of circum-Antarctic waters. It includes the southern parts of South America, Africa, and Australia, and embraces practically all of the Atlantic, Indian, and Pacific oceans within the Southern Tropic. Bathymetry is shown by a large number of soundings and by means of layer coloring in nine tints, buff for the continental shelf and eight shades of blue for the oceanic depths. Maps 7, 8, and 9 are all three on the same scale and are therefore directly comparable among each other as to area. Arctic and Antarctic land relief may be compared on maps 7 and 9, although the latter, owing to the insufficient data available, indicates elevation in restricted areas only; the bathymetry of the Arctic Sea and of the circum-Antarctic waters may be compared on maps 8 and 9; indeed these two maps constitute companion tools in the study of polar oceanography.

Note: Items 3, 8, and 9 together constitute Special Publication No. 11. Items 1, 2, and 3 together provide a selected bibliography covering the whole range of polar literature.

Remarks by Henry G. Bryant

I am sure I voice the sentiments of this audience—which is only limited by the capacity of our room—in extending to Dr. Bowman our thanks for his illuminating discussion and analysis of the recent Antarctic expeditions. In the past, this society has always been interested in polar exploration. Benjamin Franklin was a member of the committee which sent out the earliest American expedition to discover the Northwest Passage in 1752–3 and Dr. Elisha Kent Kane was one of our honored members.

In view of the present interest in the revival of Antarctic research, it should not be forgotten that our society made a

determined effort some twenty years ago to induce our Government to send out an expedition to reëxplore Wilkes Land. In response to resolutions passed at the General Meeting in April 1909 eleven other learned societies sent their endorsements of the project. A committee under the Chairmanship of the late Mr. Edwin Swift Balch visited Washington and secured favorable coöperation from Admiral Pillsbury and the Secretary of the Navy. However, after all our efforts and after estimates of the expense of the expedition had been prepared our hopes were shattered by the decision of President Taft that it was not advisable to ask Congress for an appropriation for that purpose. However, all this is ancient history, and I for one rejoice to have lived to see the United States reënter Antarctic exploration with the finely equipped expedition under Admiral Byrd. We are especially indebted to Dr. Bowman for having pointed out, with the data at his disposal, some of the scientific results attained in cartography, meteorology and geology—results which constitute real contributions to knowledge and which are sometimes overlooked in our admiration of Admiral Byrd's daring and spectacular flight to the Pole.

The fine achievements of Sir Hubert Wilkins show what can be accomplished by the daring and resolution of one man with a modest equipment, and, if Sir Douglas Mawson is also successful in his program, the results of all these undertakings will be to place an immense amount of material in the hands of experts which will result in enhancing our knowledge of the physical conditions which characterize the southern hemisphere.

With the return of Admiral Byrd in the spring, we may anticipate that there will be renewed discussion of the results of his work in the South. In view of this, we must consider ourselves fortunate that we had the privilege of hearing Dr. Bowman's admirable exposition of these results, so that we also may be in a position to discuss them intelligently. Altogether, I regard this a banner meeting of our society, and for its success we must again express our obligation to Dr. Bowman.

STUDIES ON WEATHERING AND SOIL FORMATION IN TROPICAL HIGH ALTITUDES*

By M. W. SENSTIUS

(*Read by title, February 7, 1930*)

SYNOPSIS

THE investigations reported upon in the following pages were undertaken to test the validity of modern conceptions of soil-formation, or rock-weathering in the wider sense, in the Oriental tropics.

These modern conceptions and their historical development are reviewed in Chapter I.

Briefly summarized the prevailing idea in modern agrogeology is that the soil is a function of the climate, and particularly of the climatic factors temperature and humidity. The nature of the parent rock is considered of secondary importance; it may impress its characteristics upon the resulting soil only so long as the latter is not mature ("Azonal" soils according to Dokuchaiev and Sibirtzev, "endodynamomorphic" soils according to Glinka, or "Ortsböden"—local soils—according to Ramann). In the mature stage of soil formation, however, the characteristics of the parent material have become obliterated and the resulting soils are expressions of the prevailing climates, in other words, the same climate produces the same general soil-characteristics, irrespective of the soil-forming parent materials.

The following investigations are mainly concerned with those mature soils which have been variously termed, by the originators of these modern conceptions, "Zonal" soils (Dokuchaiev and Sibirtzev), "ectodynamomorphic" soils (Glinka) and "Climatic" soils (Ramann). They will henceforth be referred to as climatic soils or major soil groups.

The direct stimulus for the inquiry came from reading E. C. J. Mohr's treatise on "The Soils of Java and Sumatra" (in Dutch, Amsterdam, 1922). In this book, the result of some twenty-five years of soil-studies in the Netherlands East Indies, the author has shown that the leading principles of modern views on the relation between climate and rock-weathering, respectively, soil-formation, are well demonstrated in that part of the Eastern tropics.

A summary of his ideas is given in Chapter II. But neither in the aforementioned book, nor in his other writings has Mohr substantiated

* This paper was awarded the Walker Prize for 1929 by the Boston Society of Natural History.

his views with analytical data. Subsequent search in the literature for such pertinent information revealed its total absence.

The ideas, however, are rationally sound and of farreaching importance in their possible application to other tropical regions. It was therefore considered worth while to independently investigate the question both in the field and in the laboratory. It was argued, furthermore, that if Mohr's views are correct, his findings should be capable of confirmation in similar regions as far to the north of the equator as his field of observations was to the south of the equator. Hence the fieldwork was carried out both in the Netherlands East Indies and in the Philippine Islands, or specifically, on the islands of Java (N. E. I.), Negros and Luzon (P. I.).

Chapter III states the problem more specifically and also describes the methods of investigation in the field and in the laboratory.

Chapter IV discusses and summarizes the results obtained.

CHAPTER I

Development of our Modern Conceptions of Weathering and Soil Formation

Everybody knows what a soil is although no one, as yet, has been able to define a soil in terms that are wholly acceptable to every one else. Agronomists have one definition, or rather a number of definitions differing in minor details, and so have geologists; it would serve no useful purpose to enumerate them here. But agronomists and geologists alike agree at least that a soil, essentially, is made up of mineral fragments which are more or less loose and were derived, by processes of weathering, from rocks, which themselves may have been solid or fragmental at the start. This mineral matter is usually mixed with more or less organic matter and water, besides air.

The obvious fact that most soils are essentially derived from rocks has led the earlier soil-investigators—be they geologists or agronomists—to build up a soil-classification on the basis of the parent-rock. Thus they spoke of limestone-soils, granite-soils, basalt soils, etc. Or else, they based their classification on the geological formation which gave rise to the

particular soils, etc. These classifications which have persisted more or less to this day are rather fully discussed in Glinka's book[1] to which reference may be made. They originated in earlier times when investigators were largely confined to more or less local observations in areas of small extent—mostly in their own neighborhood—due to the lack of the proper means for traveling far and wide from their immediate surroundings.

As traveling became easier and more scientists could embrace large areas of the earth in their explorations, they could not fail to notice that in many parts the soils were remarkably the same over extensive areas, irrespective of the parent-rock from which they were derived. Thus they found, as a rule, in the tropical lowlands, that the soils were dominantly red in color in spite of the fact that the parent-rock showed a great diversity of composition, such as basalts, gneisses, granites, limestones, shales, etc. In other parts of the earth, in the colder climates near the poles or in high latitudes, they noticed that the soils were dominantly light in color, light-gray to white, underneath a more or less thick layer of humus,—again irrespective of the rock which gave rise to these soils.

It was then that some enlightened scientists turned to the processes of soil-formation, *i.e.*, the weathering of rocks, as a possible basis for a classification of soils.

Rock-weathering refers to the action upon rocks of the weather, or, in a larger sense, of the climate, as being the average of weather-conditions over a more or less prolonged period of time. These considerations led to our more modern climatic classification of soils, a natural and genetic classification which promises to become the foundation upon which the superstructure of the whole study of the soil is to rest, in much the same way as the natural classification of plants or of animals has become the starting point for any study in botany or zoology.

[1] The numbers refer to the numbered items of the bibliographical list at the end of this paper.

It has been suggested that these modern ideas were the result of the study of soils over extensive areas on the earth, viewed as a whole in their main characteristics. One should not wonder, then, that Russian scientists were among the first, if not actually the first, to recognize the leading principles. Theirs was a country at once vast and with a great diversity of climatic conditions. To one of them, viz., the geologist Dokuchaiev fell the privilege of extensive traveling in this great country of Czarist days during the second half of the last century. Keen observer that he was, Dokuchaiev did not fail to notice that in Russia, as the climates differed according to latitudinal belts or zones, extending, roughly speaking, in a West-East direction, so did the major soil-characteristics differ with the climates from zone to zone. Thus he conceived of the zonal distribution of soils, of the zonality of soil-regions, coinciding with climatic zones, and he became the first to attempt (1879) a natural soil-classification based on climatic agencies of weathering and soil-formation.[2] Dokuchaiev was also the first to demonstrate its practicability in the actual mapping and evaluation of soils, viz. of the Government of Nijni-Novgorod (1887), then of the Government or Province of Poltava (1890–1894) and subsequently of other provinces in Russia.[3] In this work he was ably assisted by students who subsequently carried on the work of their master, elaborating and improving it until today the Russian pedologists are unquestionably the leading and most successful exponents of the newer ideas referred to above.

Extensive travels in many parts of the earth caused another geologist and geographer, this time the German scientist von Richthofen, as early as 1886 to pronounce the statement that "the soil is mainly a function of climatic agencies." [4]

And in the United States of America it was particularly E. W. Hilgard who apparently came independently to the conclusion (1892) that climate plays a very important role in determining soil characteristics. If for nothing else he will always be remembered as the first one who clearly pointed out the differences between soils that originated in humid climates and those which originated in arid climates.[5]

It may be recalled that Hilgard, for many years had taught and worked in the State of Mississippi and at the University of Michigan. He thus became thoroughly acquainted with the characteristics of the soils of the humid section of the United States. But it was not until he had gone to the University of California, there to study the soils of the arid and semi-arid West, that he made his classical cóntributions in the realm under discussion.

The three scientists referred to above, who were all geologists at the start, may be regarded as the founders of our modern conceptions concerning the relation between climate and soil formation, respectively soil-characteristics, although they had some forerunners. Treitz,[6] for instance, states that the Russian, G. Tolstoj, in 1855, and the Austrian, L. von Liburnau, in 1866, had expressed kindred ideas. These ideas, however, do not seem to have been the starting-points for much further development, so that the real credit for having stimulated a great many workers to productive research along the new lines should still be given to Dokuchaiev, von Richthofen and Hilgard.

Their work has been amplified and extended by a host of agro-geologists or pedologists in almost every civilized country. A partial list of the most outstanding workers would include the names of Sibirtsev and Glinka in Russia; Ramann, Richard Lang and Stremme in Germany; von Leiningen and Kerner von Marilaun in Austria; Treitz in Hungary; Murgoci in Roumania; Miklaszewski in Poland; Hesselman, Tamm and Simon Johannson in Sweden; Aarnio and Frosterus in Finland; Hissink and van Baren in the Netherlands; Mohr in the Netherlands East Indies, and a large number of soil-scientists under the leadership of Marbut in the United States.[7]

As a result of their work, Dokuchaiev's original scheme has been modified and improved upon, though by no means radically. Glinka has recently given a summary of the various Russian systems of soil-classification together with a profound discussion of his own.[1] Since this work is now available in the English translation by Marbut it may suffice to refer to that source of fundamental information.

Other notable contributions have been made, particularly by Ramann and Lang, to which reference may be made here.[8]

In spite of the relatively large number of soil-scientists who are now actively at work in various parts of the earth, one should not expect the scheme to be complete, inclusive and comprehensive as yet. Pedology or agro-geology as an individual branch of science is still too young and workers are too few or entirely lacking in many critical regions.

This is particularly the case within the tropics, both Oriental and Occidental tropics.

Although countless contributions have been made toward the knowledge of tropical soils, yet, with few exceptions, they lack the necessary unified viewpoint that would enable one to survey tropical soil-characteristics as a whole. One notable exception is due to the work of the Dutch agro-geologist E. C. Julius Mohr in the Netherlands East Indies and particularly on the island of Java. The physiographic configuration of the island of Java, its macro-relief, has led Mohr to the recognition of vertical zonality in addition to the previously established horizontal zonality of rock-weathering and soil-formation. The rationale of these conceptions has been clearly stated by Mohr in the year 1913,[9] and subsequently elaborated in 1922.[10]

It is true that according to Glinka (*op. cit.*, *2b*, pp. 15–16), Dokuchaiev and some of his pupils had previously expounded kindred ideas in a series of articles in Russian which date back as far as the year 1898. There is no indication, however, that Mohr had any knowledge of their writings, or of their work. At any rate he may be safely considered as being the first to have rationalized the vertical zonality of climatic soil types in the tropics.

Mohr's ideas, which were published in the Dutch language, shared the fate of those of his Russian precursors: they remained unknown outside of their native country, viz. the Netherlands East Indies and Holland.

On account of their fundamental importance, and because they were the starting point for the author's own investigations, to be reported upon presently, Mohr's ideas are reviewed at length in the following chapter.

CHAPTER II

Mohr's Ideas Concerning the Relation Between Climate and Soil Formation or Rock-weathering in the Tropics

It has been indicated that Mohr developed the conception of vertical zonality of tropical soils as a result of his work, particularly in Java.

Java is a narrow and elongated island, lying almost parallel with the equator between latitudes 6°–8° South. A chain of young volcanic mountains of late Tertiary age extends through its longer axis from West to East. Its individual peaks, some of which are still active, reach elevations up to 3,676 meters (12,000 feet) and the island shows a great variety of topographic relief forms.

Java forms part of a world of islands, the Malay Archipelago, in which the bodies of water occupy about 84 per cent. of the total area. The marine influence on the climate is consequently most pronounced, and, together with the location so near the equator, has caused Java to possess an extremely equable climate. This is expressed in almost negligibly small ranges of temperature, both annual and diurnal.

Thus in Batavia, at 8 m. above sea-level, the average annual range of temperature is only 1° C. and the average daily range is 7.3° C. The values for Buitenzorg at 240m. are respectively 1.2° C. and 8.1° C.; Bandoeng at 730 m. has respectively 0.6° C. and 9.3° C., while the Dieng-Plateau at 2100 m. has 1.6° C. and 7.1° C. respectively.

In this equable climate the average temperature is largely a function of the elevation above sea-level, decreasing by about 0.56° C. for a rise of 100 m., so that zones of temperature may be recognized something like the following (see Table 1):

The *first* zone, occupying the hot tropical, or rather the equatorial lowlands up to about 200 meters, has practically no temperatures lower than 20° C. and its average temperature of about 25° C. is found in the soil as a constant temperature at a depth of less than one meter. The temperature of this

TABLE 1 *

Elevation		Average Temperature		Minimum Temperature		Maximum Temperature	
Meters	Feet	° C.	° F.	° C.	° F.	° C.	° F.
0	0	26.5	80.0	25.0	77.0	33.0	91.0
200	600	25.0	77.0	20.0	68.0	30.0	86.0
1,000	3,000	20.0	68.0	15.0	59.0	25.0	77.0
1,800	6,000	15.0	59.0	10.0	50.0	20.0	68.0
2,600	8,500	10.0	50.0	5.0	41.0	15.0	59.0
3,400	10,000	5.0	41.0	0.0	32.0	10.0	50.0
4,300	14,000	0.0	32.0	Snow Line		Snow Line	

* (This table represents average air-temperatures. But owing to the fact that the temperature-ranges are so small, it may be taken as representative of the soil temperatures as well.)

zone offers the best conditions for the cultivation of the lowland cultures such as rice, sugar-cane, tobacco, and most of the land in this zone is used for growing these crops.

The *second* zone, moderately warm, from 200–1,000 m. which may be called the equatorial hill-lands, has no temperature over 30° C.; the average temperature decreases to 20° C. At 1,000 m. the upper limit of successful coconut growing is reached and the wet rice fields of the lowlands also disappear. Tea and coffee can well be grown here, so far as quantity production is concerned, but for the best flavor one has to go still higher, viz., to the *third* zone, from 1,000–1,800 m. in elevation, which may be termed the equatorial highland zone. Here the temperature never reaches 25° C. and the average is only 15° C. at its higher limit. Palm trees no more grow here, but the primeval forest vegetation is especially dense with mosses, ferns and orchids growing on the trees, lianas hanging down from the trees, and dense underbrush covering the ground. Here are raised cinchona, European vegetables, roses of the temperate climates, but banana trees have disappeared.

The *fourth* zone, above 1800 m. in elevation, which may be designated the equatorial high-mountain zone, comprises only small areas, mostly the steep higher slopes of the mountains and a few high plateaus. The primeval forests gradually

become lower in height and less dense, until they finally dwindle down to low brushwood with grasses. Mosses still constitute an important part of the vegetation, but above 3,000 m. practically all the vegetation has disappeared.

It may seem strange that in a discussion of soils so much attention should be paid to the vegetation. It may be justified, however, on the ground that if such relatively small variations in temperature can bring about such great differences in the macro-flora, we might well suppose similarly great differences to exist in the micro-flora, especially in the micro-organisms living on and in the soil. It is well known, for example that many species of bacteria only begin to grow well and to multiply most rapidly above 30° C.; at 20° C. their growth is appreciably arrested and becomes practically nil between 15° C. and 10° C. The higher plants, on the other hand, have a much lower optimum temperature. The significance of these considerations will be shown presently.

The rainfall of Java, which is under the influence of the monsoon-winds, is high all the year in certain parts, periodically high and low from season to season in other places, and in still others may be constantly low. (In general it may be said that the precipitation decreases as we go eastward.) Accordingly, with respect to the moisture conditions, the following three cases can be distinguished as bearing directly on soil formation:

(1) Regions where the precipitation constantly exceeds the evaporation. $P > E$.

(2) Regions where during part of the year (wet season) precipitation exceeds evaporation, and evaporation exceeds precipitation during the remainder of the year (dry season). $P \gtrless E$.

(3) Regions where the precipitation is constantly less than the evaporation. $P < E$.

The significance of these distinctions will be spoken of again presently.

A combination of the temperature zones with these humidity provinces gives twelve possible cases; in reality five of

them are non-existent in Java. For in the highland and high-mountain zones it never happens that, for any length of time, evaporation exceeds precipitation; hence four possibilities automatically drop out. In the hill lands it may sometimes happen that the dry monsoon is quite pronounced, but it is always followed by a rainy season, so that continuous excess of evaporation over precipitation never occurs here.

There are, then, only seven possibilities to be reckoned with, indicated by the + sign on the following table.

TABLE 2

Elevation	Designation	$P > E$	$P \gtrless E$	$P < E$
0–200 m. (0–600 ft.)	Lowlands	+	+	+
200–1,000 m. (600–3,000 ft.)	Hill lands	+	+	–
1,000–1,800 m. (3,000–6,000 ft.)	High lands	+	–	–
> 1,800 m. (6,000 ft.)	High mountain lands	+	–	–

It is being more and more recognized that in a mature natural, undisturbed, residual soil the nature of the parent rock plays a subordinate role. But for completeness' sake it may be stated that the underground of Java consists of geologically recent formations. These are mainly composed of sedimentary rocks—such as limestones, sandstones and conglomerates and breccias of basic igneous rocks—besides an abundance of recent igneous rocks, such as diorites, diabases and gabbros among the oldest rocks, and andesites and basalts among the youngest. All of them, then, are basic igneous rocks, *i.e.*, contain a large amount of lime and ferro-magnesian minerals, which weather readily. Since, moreover, the recent and numerous volcanic eruptions have mostly been of the violently explosive type, the soil everywhere contains pyroclastics of these basic igneous rocks in its mass.

The significance of the ratio between precipitation and evaporation in the processes of weathering, soil formation and transformation has been most clearly brought out by Hilgard.[5]

Where the precipitation exceeds the evaporation, a constant downward movement takes place of the water in the

soil, provided texture and structure of the soil permit any percolation. This effects a carrying downward and away of the soluble mineral substances, a leaching out of the soil. The soil-water, thus carried away, does not only take mineral substances into solution, but organic matter as well, more or less organic matter according to the richness of the vegetation on top.

When the evaporation exceeds the precipitation, then, the rainwater may temporarily soak into the soil for a few inches or more, only to be drawn up to the surface again and evaporated, as a result of which any dissolved material is deposited on top or near the top.

In the first case the soil is made poorer by leaching, in the second case the soil is enriched by concentration.

In the intermediate case, when $P > E$ during part of the year and $E > P$ during the remainder, there will be downward leaching to greater depth followed by upward enrichment, the one process alternating with the other.

It was said a little while ago: "provided soil texture and structure permit of such movements." Textures and structures may be:

(1) very coarse and pervious for water. If so, then, even in the case when $P > E$, part of the rainwater will move downward but cannot go up again, due to the small capillary action and the movement will be continuously downward, or intermittently downward at best;

(2) or, the soil may be fine-textured and pervious. Then, the movement of the percolating rainwater under conditions of $P \gtreqless E$ will be alternately downward and upward.

(3) in the third place, the soil may have a very fine texture, such as is possessed by clay for instance, and be impervious, but exhibit only high water-capacity. In such a case, when $P > E$ the soil will fill up with moisture, but at the same time, will permit of little downward movement; when $P < E$ this soil is likely to dry out and form cracks, but still without much movement of water from below upward.

Thus it comes about that where $P > E$, leaching at times

takes place at an enormous rate, at other times is quite small. Or again, when $P > E$, soil texture and structure sometimes may bring about intermittent leaching, sometimes an equilibrium between leaching and enrichment.

Before proceeding any further with the different combinations of temperature and water-movement, it is well to consider first the relation between the temperature and the vegetation, spoken of a while ago.

It is well known that microscopic organisms, in general, have a higher optimum temperature than have the higher plants. The lower limit of temperature for the growth of higher plants is reached somewhere around 0° C., as is exemplified by certain Alpine plants which pierce their flowers through snow and ice in the beginning of the Spring season; the upper limit lies around 45° C. or 115° F., as in the case of desert plants, and the optimum temperature is about 25° C. or 77° F.

For micro-organisms, however, these limits are different; at 15° C. or 61° F. their life activities are little intense, and at 10° C. life activities and growth have practically ceased; their optimum temperature, accordingly, is much higher than 25°–30° C., indeed it is as high as 35°–40° C., while some micro-organisms may live and thrive well in temperatures as high as 80° C. or 176° F. and perhaps even higher.

It may be roughly said that higher plants make organic matter out of water from the soil and carbon dioxide from the air, and lower organisms break down again, destroy, what the macroflora has built up. The difference between what higher plants build up and lower organisms break down constitutes that complex entity which is roughly termed "humus."

The important question, is: when and where is humus formed and when and where is humus destroyed; in other words, when and where is humus accumulated, when and where is humus absent, destroyed faster than it is being formed?

Humus formation takes place where a luxurious vegetation prevails, hence in forests, at the proper temperature, or, still

better, where abundant sunshine fosters vigorous assimilation by higher plants, *i.e.*, in the hot equatorial lowlands (as well as in the lowlands of higher latitudes, even if the temperature here has become much lower). Also in the more elevated, cooler highlands of such countries like Java, where the sun stands overhead almost all the year round.

While higher plants need much sunshine, much insolation, the lower organisms on and in the soil, for their best development need heat most of all, next to air and water.

Thus, humus is destroyed most rapidly where the temperature is high and is continuously high, *i.e.*, in the lowlands and in the hill lands of the equatorial regions.

So, humus can be maintained only and can accumulate only where the conditions for its formation are more favorable than the conditions for its destruction, in other words, where higher plants thrive better than the lower organisms. This can best be read from the following diagram.

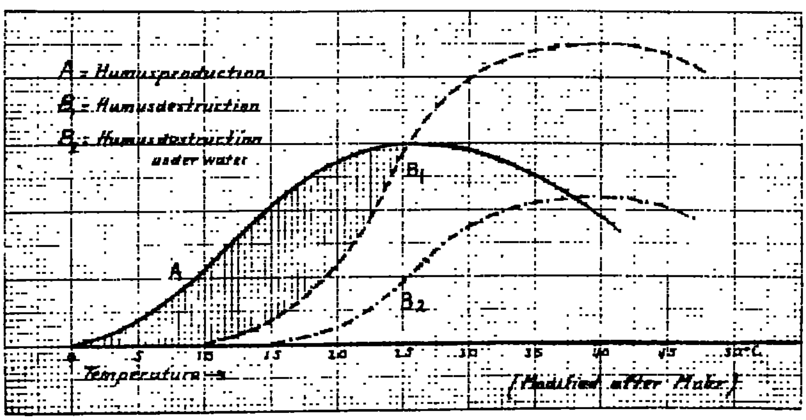

DIAGRAM I

Thus the conclusion may be drawn, that in humid and hot equatorial regions, with an average temperature of 25° C. and over, humus cannot maintain itself or accumulate in a properly aerated soil.

As the percolating soil water comes in contact with humus, it will dissolve organic substances, and become humus laden

water, brown in color. This, of course, happens only where humus can persist more or less; where humus cannot persist, but is burnt up into carbon dioxide and water, there is no organic matter in the soil-water, but rather carbon dioxide in its stead.

Hence the following conclusions seem justified:

(1) In humid and hot equatorial regions, where the average temperature is 25° C. or 77° F. and over, the water in the soil does not contain organic matter in solution, provided the soil is well aerated;

(2) Humus-laden water is found only in the cooler mountain zones of the equatorial regions. In addition, humus-laden water is found also in the lower, hotter zones where the proper supply of air is lacking, *i.e.*, in poorly aerated soils.

This last statement, perhaps, requires some elaboration and explanation.

In soils that are permanently under water, *i.e.*, in marshy and boggy ground, water has largely replaced the air and the soil is lacking in proper aeration. Under such conditions the higher vegetative life may take on another aspect than on the well-aerated soils, yet it is well-known that the luxuriance of the vegetation is but little impaired. Humus formation goes on at much the same rate, so curve *A* in the diagram above needs hardly to be modified. But the micro-flora under such conditions suffers proportionately much more from lack of air, and consequently the total destruction of humus into carbon dioxide and water is much reduced. Hence, if we represent the intensity of humus destruction under these newer conditions by a curve, we would get something like curve B_2. It should be noted that this curve crosses curve *A* beyond the limit of importance, if at all. The significance of this should be immediately clear: in water-logged soils humus-accumulation takes place at all temperatures, and the ambiant water is humus-laden, brown in color, much the same as such boggy water is in our own middle latitudes.

These conditions affect rock-weathering most profoundly

when it is considered that silica, one of the most abundant constitutents of rocks and soils, is relatively most soluble in pure rain-water; the presence in solution of salts, of carbon dioxide or of humus decreases its solubility.

Kaolin, the basis of all that is called clay, is likewise more soluble in pure water than in water containing salts or carbon dioxide or organic matter.

However, iron-oxide and aluminum oxide, other important constituents of soils and rocks, are less soluble in pure rain-water and salt solutions, but are rather easily soluble in humus-laden water.

So it is apparent that if leaching takes place by water free from organic matter, then free silica and even kaolin will be removed in the long run, but iron-oxide and aluminum oxide tend to persist, while leaching by humus-laden water ends up in products that are rich in silica, but poor in iron-oxide and aluminum oxide.

Thus the main types of weathering and soil formation in Java may be summarized as follows:

DIAGRAM 2

		$P > E$	Alternate Wet and Dry Seasons		$P < E$
Direction of movement of water in the soil.................>		↓ ↓ ↓ ↓	↓ – ↓ – ↓ –	↓ ↑ ↓ ↑	↑ ↑ ↑ ↑
Weathering above ground-water level, i.e. under aerobic conditions	Temperature high	(1) yellow red	(2) red	(4) black	(5) gray
	low	(3) white	—	—	—
Weathering under water, i.e. with lack of air	Temperature high	(6) gray-white	—	(7) black	—

Type 1: temperature high; precipitation always greater than evaporation; soil material loose and pervious, above ground-water level, well aerated—constant downward

leaching by warm rainwater with no organic matter in solution.

It is being recognized more and more that everywhere on the earth this type of weathering will result into end-products which are remarkably uniform, whatever parent-rocks may have furnished the material. Those end-products consist of little else but iron oxide and aluminum oxide, but when the original rock contained quartz and other similarly resistant minerals, then, of course, we would find these little altered as admixtures.

These products of weathering have generally been called "laterite." But as this term "laterite" has been derived from the color, viz. from the color of red brick, which it resembles, and does not convey any knowledge about its mode of formation, and since the same word has given rise to so many misinterpretations and so much misuse, it is desirable to replace it by a more suitable name. Mohr has proposed the term "lixivium" meaning "what has been leached out," to supplant the term "laterite." Under these conditions of weathering iron oxide is accumulated as yellow to brown iron hydroxide, which imparts a yellow to brownish color to the whole mass, hence giving rise to yellow or brown lixivium. In time the hydroxide may naturally become dehydrated and the soil becomes a red lixivium.

Type 2: temperature high; rainy season alternating with more or less dry season; soil material very pervious, above the groundwater level, well aerated; intermittent leaching and enrichment by warm rainwater without organic substances in solution.

This type of weathering again results into a lixivium, poor in silica, rich in iron oxide, which is dehydrated during the dry season, and does not become hydrated again during the wet season. Hence a dominantly red soil, a red lixivium, formerly called a red laterite also, but in reality quite different from the preceding red laterite.

Type 3: temperature low; rainfall always greater than evaporation; constant leaching by cool water, containing more or less humus.

This type of weathering, resulting in a "podsol," such as is most typically exhibited in Canada, Northern Michigan, and Northern Europe, is hardly found in the high mountain and plateau regions of Java. Instead, intermediate stages between type 1 and type 3 are met with. Higher up on the mountain slopes the amount of humus increases, and the percolating soil-water becomes increasingly richer in dissolved organic matter. Under such conditions iron will be removed, whereas silica will persist and increase proportionately. The result is a light colored soil, almost white, resting under a layer of black humus. This light colored soil then is a white "lixivium" and is so called.

Type 4: temperature high; rainy seasons alternating with dry seasons; soil material moderately pervious, but with good capillarity; alternate leaching by warm water with little humus, and enrichment by evaporation of ascending groundwater containing dissolved salts and more or less humus.

In this type of weathering the downward percolating water during the rainy season carries large quantities of carbon dioxide resulting from the destruction of organic matter in the hot season. This water, when drawn upward during the dry season, now contains in solution calcium bicarbonate, besides sodium, potassium, magnesium and iron carbonates. Upon evaporation of the solvent these substances accumulate on or near the surface, where they react with the organic matter, imparting a black color to the soil. Hence a black soil, but not the same shade as the black humus soil of the cooler climates, instead a bluish black to grayish black, as distinct from the brownish black or dark brown of pure humus soils.

Another characteristic feature of these soils is the presence of concretions of lime and of hydrated iron oxide. They are formed at a depth down to which the soil cracks during the dry season, hence down to which aeration takes place.

In the hill zone, higher up than the hot lowlands, the temperature is somewhat lower, and the lack of rainfall during the dry season is less pronounced. Here transitional stages be-

tween 4 and 3 are found, as indicated by the gradual change in color from grayish black and bluish black into brownish black and dark brown.

Type 5: temperature high; precipitation always less than evaporation; alternate wetting and drying out; downward movement of rainwater for a short distance or in occasional cracks, followed by upward movement of salt-bearing water; enrichment by evaporation at the surface (alkali soils).

Such a constantly dry climate, resulting in desert conditions, fortunately does not occur in Java; it may perhaps be found farther eastward, on the adjoining islands of the Malay Archipelago, or in Australia, but so far only small indications of the formation of surface crusts of lime carbonate, gypsum, and rarely other compounds have been found in Java, showing transitional stages between 5 and 4.

Type 6: temperature high; precipitation constantly higher than evaporation; soil permanently under water, continued leaching under water and by humus-laden, warm water.

This type of weathering which may be called subhydric or subaquatic or under-water weathering, results in a rapid and intensive leaching-out process, producing light gray soil, the "Glei" soils of the Russians, which may, at times, have a tinge of darker gray or bluish gray; they contain little else but kaolin and silica and perhaps ferrous sulphides.

The vegetation of such soils, constantly under water, will thrive mostly on whatever nutrients are brought in from elsewhere by the inflowing waters.

Type 7: temperature high; climate with wet and dry seasons; leaching by humous water during the wet season; concentration and enrichment by ascended alkaline water containing organic matter and salts during the dry season.

In this type of weathering which is found in alternately wet and dry swamps, a black soil is formed, less alkaline than (4)

or (3), less leached out than (6) but with more organic matter of the same quality as described from (4). Iron oxide is here characteristically found in concretions again.

CHAPTER III

The Problem Stated. Field and Laboratory Methods

The essentials in the foregoing discussion may be once more brought out before our particular problem is stated. Mohr has given a reasonable explanation for the reason

(1) why in the tropical lowlands, with abundant rainfall, any kind of rock that weathers under conditions of a proper supply of air and water should eventually become a laterite or a bauxite, both of which being composed of little else but iron and aluminum oxides or hydroxides, usually red in color;

(2) why in the tropical lowlands, with alternate wet and dry seasons, the typical black rice-soils should be developed;

(3) why in the tropical high-mountain zones, with abundant rainfall and middle latitude temperatures, a type of weathering and soil-formation should be found which corresponds to that of the middle latitudes and perhaps even to that of high latitudes, *i.e.*, the type of weathering which is now referred to as "podsolization" and eventually ends up in the formation of a "podsol."

The process of laterization in tropical lowlands has been the subject of countless investigations by scientists of almost every nation since 1807, at which time Francis Buchanan (-Hamilton) gave the name "laterite" to a remarkable, ferruginous residual rock which he had met with on his travels through Southern India.[11]

Our knowledge of the subject has recently been ably summarized and clarified in the works of Lacroix, Harrassowitz and Fox[12] so that it was not considered expedient nor necessary at this stage to add to the voluminous literature on the problem.

The question of tropical black soils has also received a great deal of attention particularly from the side of resident

British investigators in India and South Africa, and will not be treated at this time.

The problem of podsolization in the tropics, however, has not been studied, so far as the author could determine, except by Mohr. But even he has not systematically set out to study characteristic profiles at definite elevations, nor has he substantiated his theories by the publication of the results of laboratory studies.

Other investigators in the tropics, whether resident or transient, have not yet given the matter any attention, so far as the writer could determine. For these reasons and because of its importance as a test case for modern conceptions on the climatic control of weathering and of major soil characteristics, it was considered worth while to independently investigate the problem in the field and in the laboratory.

This paper, being the result of such investigations, can be regarded as the first attempt at a systematic orientation into the problem of weathering and soil-formation in equatorial high altitudes, specifically on islands of the Oriental tropics.

The Problem Stated

The object of this study is to ascertain whether podsols are found, respectively whether podsolization takes place in high altitudes in the tropics.

Podsol is the typical soil of large areas in the Northern hemisphere where the climate is humid and cold, yet not too cold for the development of a luxuriant vegetation, mostly of the forest-type, so that plenty of organic matter or humus can be accumulated. The characteristic profile of a podsol shows a dark humous layer of variable thickness on the top, or in the A_1-horizon so-called. Below it lies an ash-gray layer—the A_2-horizon—which owes its light color to the removal of coloring matter, such as dark-colored mineral constituents, but mainly the hydrated iron oxides, under the influence of humus-laden percolating waters. A residue of light-colored resistant minerals is left, consisting principally of silica or quartz and muscovite and hydrated aluminum oxides, etc. The iron

oxide migrates downward, enriching the iron content of lower *B*-horizons, and may, under certain conditions, to be discussed later, be precipitated to form a "hardpan," or a layer of so-called "bog iron ore."

In case this final stage of podsolization has not been reached, the soil is spoken of as a podsolized soil. It will then show varying degrees of "lightness" of color of the A_2-horizon and increasing iron content from the top of the *B*-horizon downward.

It is evident that the development of a podsol, or podsolization in general, is favored by

(1) a loose and pervious, fragmental parent material;
(2) an abundance of precipitation all the year around;
(3) an even and low temperature all the year around;
(4) a luxuriant primeval forest vegetation.

These conditions are admirably found on many islands of the Oriental tropics, notably on most of the larger islands of the Malay and Philippine Archipelagoes.

The majority of the high mountains in this part of the earth represent cinder and composite cones of extinct or still active volcanoes. The volcanic eruptions which were and still are of the explosive, Vulcanian or Vesuvian type, have given rise to loose and porous deposits, often of great thickness, on the flanks of the mountains and beyond. These ejectamenta or "efflata"—a new word suggested by Mohr, denoting "what has been blown out"—are the usual buff-colored pyroclastics or tuffs of basic igneous rocks that are characteristic of the great majority of the volcanic tuffs of recent Vulcanian eruptions.

There is one serious drawback, however, to the mountains just spoken of, viz., the fact that old soils have often received and are still receiving fresh volcanic dust as the result of eruptions of nearby or even distant volcanoes.

The case of Krakatoa volcano may simply be recalled in this connection. Consequently a great many of the collected samples proved to be unsuitable for the present investigation and had to be discarded.

Field Methods

Since topographic maps are wanting for many of the mountains to be climbed, the author carried a reliable aneroid barometer for determining elevations. This barometer, made by Richard Frères in Paris (France), was the large precision type with a diameter of 9 cm., compensated for temperature changes and reading from -200 m. to 5,000 m. with intervals of 20 m. The instrument was compared with the ship's mercury-barometer three times daily for a period of three weeks on the voyage from Marseille to Batavia, and was found to be accurate. It was tested again and found accurate by the Royal Magnetic and Meteorological Observatory at Batavia (Java) and once more, with the same result, at the Manila Observatory in the Philippine Islands.

In equatorial regions, particularly those with a marine type of climate, the variations of barometric pressures during the day and from day to day are regular, besides negligibly small. Consequently the determination of elevations in our fields of operation by means of a reliable aneroid barometer may be considered sufficiently accurate, at least for our purposes.

The soil-observations were made on natural cuts or in test-pits especially dug for the purpose. These pits were about 150 cm. long, 50 cm. wide and at least 100 cm. deep. One wall was made smooth and the observations were made on this wall. These observations included (1) the thickness of each distinguishable horizon; (2) its color; (3) its texture and structure. Furthermore, the reaction of each horizon was determined separately and in place by means of the "Soiltex" method according to Spurway, which expresses the hydrogen-ion concentration sufficiently accurately for all practical purposes.* Soil samples of convenient size were then taken from each horizon separately at definite depths. These samples, duly labeled, were put in cloth bags and dried in the shade at the earliest convenience.

* The "Soiltex" indicator-liquid is an aqueous solution of bromthymol blue (Clark and Lubs); it gives a deep blue color with alkaline, and a varied range of yellow colors with acid solutions.

Laboratory Methods

In the laboratory the samples were passed as air-dry soil through a sieve with round holes of 2 mm. diameter, according to the usual methods in soil-analysis. It was often necessary, in this operation, to rub the soil in a mortar with rubber-tipped pestle in order to break up lumps but not stones and gravel. Macroscopic bits of plant-roots and pieces of charcoal were removed as much as was possible.

The part which passed through the 2 mm. sieve, the "fine earth," was taken as the basis for all subsequent determinations, viz., the mechanical, mineralogical and chemical analyses.

The mechanical analysis was carried out according to the standard methods of the United States Bureau of Soils,[13] in duplicate sets of five grams of the oven-dried material, and the weights were recorded to the third decimal place. The clay content was computed from the difference in weight between the original sample and the sum of total silt and sand. Difficulties were encountered with samples high in humus-content so that several analyses were repeated and an average computed.

The mineralogical analysis was carried out on the sand-fractions separated by means of the mechanical analysis. The methods of analysis were mainly petrographic-microscopical, aided by separation with acetylene tetrabromide of specific gravity 2.95.

The chemical analyses were carried out partly by the writer himself insofar as the laboratory facilities permitted, but mostly by Drs. B. A. Soule and R. K. McAlpine of the Department of Chemistry, University of Michigan, and by Mr. Earl V. Shannon, of the United States National Museum at Washington, D. C., to all of whom the writer may here express his cordial thanks for their coöperation.

Complete analyses, after fusion with sodium carbonate, were made according to the standard methods for rock-analysis.

The fusion method was resorted to rather than the extraction methods by means of strong hydrochloric acid and

strong sulfuric acid, which are more in favor with agricultural chemists. This was done because the problem was considered a geological rather than an agricultural problem, the soils being regarded as geological phenomena per se rather than as a support for plant-life.

The usual elements called for in a complete rock-analysis were thus determined, except ferrous iron, which could not be determined in the presence of so much organic matter. Because of its importance in our considerations the organic matter or humus was determined additionally in three ways:

(1) as loss on ignition;
(2) as alkali-soluble humus—the "matière noire" or "black matter" of Grandeau;
(3) as total carbon.

Ad. 1.—The loss on ignition was determined by the chemists referred to on the basis of the sample as received, *i.e.*, in the air-dry condition, by heating over a Méker burner in a platinum crucible. The author made his determinations on the basis of the soil dried at 100° C. and heated in a porcelain crucible at low red heat over an ordinary Bunsen burner, *i.e.*, the customary way in soil-analysis.

Ad. 2.—The "soluble humus" or "black matter" was determined by the author according to the method used in agricultural-chemical analysis, *i.e.*, solution in 4 per cent. ammonia by weight after decalcification with 1 per cent. HCl and subsequent washing out of the acid.[14] An aliquot amount was then drawn off from the filtered solution, evaporated, weighed, ignited and weighed again. The difference in the last two weights represents the alkali-soluble humus or black matter.

Finally it was considered necessary to determine independently the amount of iron hydroxide adsorbed by the soil particles, because of its great importance in the consideration of the migration of iron oxides in the soil. This absorbed iron hydroxide is referred to as colloidal Fe_2O_3 in the tabulation of the analytical results.

Its importance is derived from the fact that, in a recent

physical-chemical study of certain French soils, Demolon has shown that up to 88 per cent. of the total iron oxides ("limonites") in a soil may be so adsorbed on the surface of the soil-particles.[15] He has also found a "rapid" method for its determination which has been essentially followed in our determinations. Since the procedure does not appear to have become known in this country, it is considered worth while to describe it here in full (Demolon, *op. cit.*, pp. 48–49). To 2 g. of soil in an Erlenmeyer flask are added 100 cc. of a 2 per cent aqueous solution of oxalic acid. If the mixture effervesces, which happens when the soil contains more than 0.40 per cent. of alkali-earth carbonates, more powdered oxalic acid is added to correspond with the decomposition by the carbonates. Let the mixture stand for 48 hours, then add 10 cc. of an ammoniacal citrate solution, shake and let digest for 15 minutes. (This ammoniacal citrate solution is prepared by dissolving 400 g. citric acid in concentrated ammonia of 22° to make up 1 liter.)

The mixture is then brought to a standard volume ("on jauge") and filtered; one half is drawn off (representing 1 g. of material) and transferred to a precipitating flask. Add a few drops of H_2O_2 and boil. Add gradually 20 per cent. KOH and heat until the ammonia is eliminated with the complete separation of the iron in floccules. Filter, wash and redissolve the precipitate in a boiling solution containing 12 per cent. by volume of strong HCl of 22° Bé. Titrate with standardized sodium thiosulfate solution containing 5 g. per liter in the presence of sodium salicylate and a small quantity of copper sulfate which acts as a catalyzer, according to Landolt (*Bull. Ass. Chim. d. Sucrerie*, 1921, 8, 477).

The soil sample must not have been heated or ignited because "limonite" that has been heated to as low as 200° C. is no longer soluble in oxalic acid. Previous decalcification with dilute HCl—1 per cent. is ordinarily used—should also be avoided, for it always results in dissolving a small quantity of iron.

CHAPTER IV

Results of Field and Laboratory Work

The field observations bore out, in general, the findings of Mohr as they have been discussed in Chapter II. This was true for the Dutch East Indies as well as for the Philippine Islands.

In the lowlands the soils were found to be dominantly red in color with no evidence of organic matter in them. They were sticky and stiff when wet, hard and cracked when dry, thus showing the characteristics of clay without humus. The rice and marsh soils (Mohr's type No. 7), next to the red soils the most important so far as their extent is concerned, were generally found to be black or grayish-black in color, equally stiff and sticky when wet and equally prone to become hard and to develop cracks when dry. However, black soils rich in humus were also noticed in the lowlands where they developed under well-aërated conditions, even in sands. But they were found only under a grassy vegetation, viz., under the kind of tall and coarse grass which is called "alang-alang" or "lalang" in the Dutch East Indies and "cogon" in the Philippine Islands (Imperata arundinacea).

This is a problem, the explanation of which requires more study and does not concern us here. A similarity may be suggested between the processes of organic matter preservation on tropical grassy plains and on the prairies of the middle latitudes.

As one went up the mountain slopes, humus began to appear under forest cover and increased visibily as one went higher and higher. But the dominant color of the soil remained red, though not the bright brick-red of the lowlands, but rather a brownish-red, over chocolate brown and chestnut brown to dark blackish brown, due to the admixture of increasing amounts of humus—except in the case of fresh to fairly fresh volcanic tuff soils. Their buff color naturally persisted the longest on the slopes of the active volcanoes as it did often also on the slopes of an extinct volcano in close

proximity to an active one. These phenomena were particurarly well exhibited on the slopes of the twin volcanoes Merapi (active) and Merbabou (extinct) in Central Java, whose individual peaks are about 10 km. apart.

Soil textures and structures also changed as one went up the mountain slopes. The stiff impervious clay-soils of the lowlands gave way to a loose, friable and porous sand, often remarkably uniform to great depths—except in the case of young volcanic efflata soils. These became rapidly coarser the more closely they were deposited near the volcanic center of eruption and often showed layers of greatly varying texture over a small range of depths.

Those facts spoilt the use, for the present purpose, of all the samples collected on the active volcanoes and of many of those from extinct or dormant volcanoes as well. They represent immature stages of soil-development, they are "Skelett-böden" or skeleton-soils, according to Ramann, constituting one of the many forms in which "local" (Ramann) or "endodynamomorphic" (Glinka) soils may appear; they clearly are not "climatic" (Ramann) or "ektodynamomorphic" (Glinka) or "zonal" (Dokuchaiev, Sibirtsev) soils and hence will not be mentioned in the following discussions.

In all, a dozen mountains were climbed in order to collect the field observations and the representative soil samples. Eight are located in Java and four in the Philippine Islands as follows:

TABLE 3

Name	Location	Kind	Elevation
Mt. Andjasmoro..	East Java	Extinct volcano	2,342 m. (7,760 ft.)
Mt. Kelout......	Central Java	Active "	1,731 m. (5,680 ft.)
Mt. Lawou.....	" "	Extinct "	3,265 m. (10,700 ft.)
Mt. Merbabou....	" "	" "	3,145 m. (10,300 ft.)
Mt. Merapi.....	" "	Active "	2,910 m. (9,550 ft.)
Diëng Plateau..	" "	Fumarolic region	2,100 m. (6,890 ft.)
Mt. Malabar..	West Java	Extinct volcano	2,343 m. (7,680 ft.)
Mt. Gedé.....	" "	Active "	2,962 m. (9,710 ft.)
Mt. Canlaon..	Negros, P. I.	" "	2,438 m. (8,000 ft.)
Mt. Banahao....	Luzon, P. I.	Extinct "	2,188 m. (7,180 ft.)
Mt. Maquiling....	" " "	" "	1,144 m. (3,750 ft.)
Mt. Santo Thomas	" " "	Structural mountain	2,258 m. (7,400 ft.)

Nowhere did the field observations show the presence of a typical podsol such as is familiar to the author from Northern Michigan and Southern Canada (Province of Ontario).

Mohr (*op. cit.*, 10, pp. 102–103) considers certain white soils which he found on the Diëng Plateau (2,100 m.) as being podsols although he calls them, by a self-coined name "alto-pallescium" (meaning: what has been bleached due to the elevation, *i.e.*, the cooler climate prevailing at this high altitude). The author, however, could establish in the field that those soils are not podsols, but have been bleached by pneumatolytic processes.

In the first place, this whole plateau abounds with features that are characteristic of the waning stages of volcanic activity, such as fumaroles, solfatara, mofettas, etc.

In the second place, the white soils were found only in distincty localized areas of small extent, here and there, but by no means regularly under the rather thick covering of muck or peat which had accumulated in the numerous swamps, nor under the humus-cover on dry land.

Thirdly, the author was enabled to see fresh exposures in cross-section for thicknesses of over 30 m., which were caused by landslides resulting from a severe earthquake of the year before. These cross-sections showed the white deposits to have a convex upward form, sharply defined from the surrounding and overlying dark-colored soil. This form is wholly consistent with the view that pneumatolytic gases, be they laden with sulfur dioxide or carbon dioxide, have diffused from an orifice below, upward through the mass of soil material, and have caused the alteration and bleaching of the soil minerals. The process is thus analogous with one of the three ways by which kaolin is known to have been formed.[16]

In the fourth place, the author found many examples of such bleaching action of volcanic exhalations going on at the present time around active fumaroles. This fact is too well known among geologists to need further elaboration.

In the fifth place, nowhere did one find accumulations into layers or concretions or "hardpans" of the iron oxide,

supposedly washed downward by the percolating humus-laden waters—a characteristic feature of most true podsol profiles.[17]

Finally, it would require a great strain on one's imagination to conceive of podsols having been formed to a thickness of 30 m. and perhaps more, considering the fact that the soil-forming material must have been only recently deposited (geologically speaking) in this region of waning volcanic activity. Thus there is no doubt in the writer's mind that Mohr's so-called "altopallescium" is not podsol as he wrongly believes.

While the true podsol was nowhere encountered in the field, yet evidence was not wanting that the process of podsolization does take place in the tropical high altitudes under consideration. Indications to that effect were found in the generally lighter coloration of subsoils under humus, such as will presently be described.

Subsequent laboratory investigations confirmed the field observations, and may now be presented and discussed.

The laboratory investigations were carried out on whole profiles, up to the depth of one meter, of carefully selected samples. Injustice would have been done to the theory if every mountain soil had been taken into consideration without regard to the elevation or irrespective of whether the soil-material was fresh or not. Hence all profiles below 2,000 m. were eliminated, and so were the ones that showed too recent an origin. This required preliminary mechanical analyses and examination of many samples that were subsequently culled out. In this way only three of the forty odd profiles passed the elimination tests, as follows:

Mt. Lawou, Central Java, W. slope at 2,800 m.
Mt. Malabar, West Java, S.S.W. slope at 2,200 m.
Mt. Banahao, Luzon, S.W. slope at 2,100 m.

Soil from Mt. Lawou, Java

This soil was collected November 16, 1925, on the west slope of Mt. Lawou in Central Java at an elevation of 2,800 m.

Climatic data are not available for this elevation, nor for any nearby station, so that they can only be surmised from the general considerations in Chapter II.

On November 15, 1925, the air-temperature at an elevation of 2,040 m. at 10 A.M., sun shining, in mountain forest, was 22° C., whereas the soil-temperature, 5 cm. deep, was 16° C. At 2,720 m., 2 P.M., the air-temperature, with sun obscured, was 18° C. and at 3,020 m., 5 A.M., the thermometer registered 5° C.

The primeval forest trees were dripping wet until about 10 A.M., and the undergrowth, particularly the mosses, was wet all the time. The clouds began to gather around noon and drizzling rain fell around 3 P.M., lasting all through the night until about 6 A.M. Yet, the nearby lowlands, at the time, were experiencing an exceptionally protracted dry season. These chance observations agree fairly well with the general considerations in Chapter II and enable us to assume an average temperatute of 11° C., at the stated elevation, with abundant rainfall all the year around.

The soil under primeval mountain forest at an elevation of 2,800 m. showed a typical profile as follows:

Horizon *A* from 0–30 cm.: a moist, loose, fine sandy, dark-brown humous layer without pebbles or stones or concretions; *pH* = 5.0

Horizon *B* from 30–100 cm.: a moist, loose, fine sandy, yellowish-gray layer; in exposed places becomes dusty, rather compact, uniform in texture, and pinkish in color; *pH* = 5.0

Samples of both horizons were taken and analyzed separately under the serial numbers 106 and 107.

In the air-dry condition the topsoil has a dark gray color and the subsoil a light buff color with a yellow cast.

The *mechanical analyses* are tabulated in Table 4. They show the *B*-horizon to be somewhat finer in texture than the *A*-horizon. The first one is a loam and the second a sandy loam according to the classification of the United States Bureau of Soils.

TABLE 4

MECHANICAL ANALYSES

	Java: Mt. Lawou W-slope 2,800 m.		Java: Mt. Malabar SSW-slope 2,200 m.			Luzon: Mt. Banahao SW-slope 2,100 m.		
Serial number	106	107	101	102	103	121	122	123
Horizon	*A*	*B*	A_1	A_2	*B*	*A*	B_1	B_2
Depth in cm.	0–30	50–70	0–15	15–35	50–70	0–20	20–25	25–50
Coarse gravel > 2 mm.	None	None	None	None	None	None	None	None
Total sand 2–0.05 mm.	58.16	48.06	84.75	84.23	85.88	82.44	86.84	84.06
Total silt 0.05–0.005 mm.	31.30	43.53	6.30	7.22	8.20	10.70	9.21	12.18
Total clay < 0.005 mm.	10.54	8.40	8.95	8.55	5.92	6.86	3.95	3.76
Class name	Sandy loam	Loam	Sand	Sand	Sand	Sand	Sand	Sand
GRADES								
1. Fine gravel 2–1 mm.	1.87	1.36	18.22	21.07	16.28	6.69	1.99	3.61
2. Coarse sand 1–0.5 mm.	13.32	3.93	29.87	23.94	17.92	17.08	14.91	14.11
3. Medium sand 0.5–0.25 mm.	10.24	6.31	16.81	15.39	13.34	19.22	22.25	18.81
4. Fine sand 0.25–0.10 mm.	18.51	15.78	14.44	16.59	22.74	27.28	32.18	30.53
5. Very fine sand 0.10–0.05 mm.	13.90	20.88	5.18	7.56	16.72	11.86	15.75	17.27
6. Silt 0.05–0.005 mm.	31.30	43.53	6.30	7.22	8.20	10.70	9.21	12.18
7. Clay < 0.005 mm.	10.54	8.40	8.95	8.55	5.92	6.86	3.95	3.76
	99.68	100.19	99.77	100.32	101.12	99.69	100.24	100.27
Moisture 100°	4.26	2.82	6.94	7.82	11.55	8.40	11.18	10.95
Ignition loss	26.45	15.32	38.29	32.87	30.67	38.04	30.45	27.23

The *mineralogical analysis* shows all the fractions of the *A*-horizon to be dark in color due to the admixture of organic matter, such as bits of plant-structures, charcoal and charcoal-like carbonaceous matter, all of which occupy the greatest bulk. The fractions of the *B*-horizon are all light gray in color with little carbonaceous matter. The mineral particles in both horizons are angular and look quite fresh. The topsoil contains about 8 per cent. (by weight) of heavy minerals (s.g. > 2.95) and the subsoil about 12 per cent. No difference was found between the constituents of topsoil and sub-soil.

The following mineral constituents were recognized: bits of andesitic pyroclastics and volcanic glass, plagioclase feldspars, quartz, iron-stained translucent chert or flint, augite, hornblende, magnetite and ilmenite and tourmaline.

The *chemical analysis* tabulated in Table 5 shows both samples to have approximately the same composition, little different from that of an andesite, such as has been reported from this volcano by Verbeek and Fennema, viz., a pyroxene-andesite.[18] It should be noted that the amounts of alkali and alkali-earth metals are somewhat lower than in most andesites.[19] Their compounds, however, are among the first to be removed in chemical weathering of any kind. It should therefore not be surprising that under the prevailing conditions of heavy rainfall the year round and a pervious substratum, the percolating meteoric waters should have noticeably decreased the amounts of alkali and alkali-earth metals. Nevertheless, the material is essentially the same as it was when deposited as volcanic tuff. Hence it has not yet become a climatic soil; the light-colored subsoil which, in the field, looked like a podsol-horizon is actually but a tuff-layer, little changed since it was deposited. This particular soil, then, is only a skeleton-soil at best and must for that reason be discarded from our discussion of climatic soil-types.

In passing, a seeming anomaly may be pointed out in the distribution of the alkali and alkali-earth metals. Ordinarily these substances increase downward in a humid soil, but in

this soil the reverse is the case. This fact becomes intelligible when it is considered that Mt. Lawou is located between two active volcanoes of the Vesuvian type, viz., Mt. Merapi, distant by about 100 km. to the west, and Mt. Kelout, about 150 km. to the east. Both of these volcanoes have had frequent eruptions in recent historical times, thereby ejecting large quantities of volcanic tuffs far and wide. Thus the

TABLE 5

Java (Neth. East Indies): Mt. Lawou; W. slope at 2,800 m.

CHEMICAL ANALYSES

Ss means determined by author;
McA means determined by R. K. McAlpine;
Columns I refer to the percentages in the air-dry sample as is;
Columns II refer to the same percentages recalculated on the basis of the mineral part = 100.

	Serial No. 106 Horizon *A* 0–30 cm.	Serial No. 107 Horizon *B* 50–75 cm.
Ss *pH*	2.0	5.0
" Moisture 100°.	4.26	2.83
" Ignition loss	26.45	15.32
" "Black matter" (alkali-soluble humus).	10.50	7.17

TOTAL ANALYSIS

	I	II	I	II
McA Ignition loss	27.65	—	16.69	—
" SiO_2	43.59	60.25	52.02	62.44
" TiO_2	0.68	0.94	0.83	1.00
" Al_2O_3	13.27	18.34	14.41	17.30
" Fe_2O_3	6.92	9.56	7.69	9.23
" MnO	0.40	0.55	0.40	0.48
" CaO	2.81	3.88	2.13	2.56
" MgO	1.37	1.89	1.35	1.62
" Na_2O	1.90	2.62	2.12	2.54
" K_2O	1.46	2.02	2.22	2.66
" P_2O_5	0.21	0.29	0.20	0.24
" SO_3	0.52	0.72	0.62	0.75
	100.78		100.58	
" Total carbon	11.19		6.82	
" Organic matter (C × 1.724)	20.52		11.76	
" Fe_2O_3 colloidal	3.19	4.41	4.09	4.91
id. *id.* (in total Fe_2O_3)		45.05		53.18

Kelout eruption of 1901 contributed a dust-layer, at least 2 cm. thick, to the nearby city of Souracarta, which is located some 40 km. farther to the west from Mt. Lawou. A still greater eruption of the same volcano took place as recently as the year 1919 and covered almost the whole island of Java with a more or less thick layer of fresh volcanic dust of the alkali-lime series of rocks. Thus the slopes of Mt. Lawou have received fresh soil-forming material on various occasions in the near past, which explains the uncommon distribution of the alkali and alkali-earth metals in the soil column.

This matter is emphasized here because the same phenomena, due to similar causes, occur also in the two other soils to be discussed presently.

J. Th. White has recently published a generalized geological map of Java on which the distribution of such deposits has been indicated as to source, thickness and extent.[20]

Soil from Mt. Malabar, Java

This second soil from Java was collected December 22, 1925, on the S.S.W. slope of Mt. Malabar in West Java at an elevation of 2,200 m.

Temperature and rainfall records are available for a nearby station, some 600 m. lower down on the same slope, viz., for the Government Cinchona Experiment Station at Tjinjiroean, 1,585 m. above sea-level.

The mean temperature, measured here over a period of ten years (1905–1915), is 16.7° C. (62° F.).[21] With a temperature gradient, according to Braak (*op. cit.*, p. 322), of 0.61° C. for a rise of 100 m. in the free atmosphere, the average annual temperature at 2,200 m. may be estimated at about 13° C. (55° F.) The average annual rainfall, as measured for a period of 44 years at the lower Station, is 2,900 mm. (115 inches) and no month has less than three inches of rainfall.[22] It may safely be assumed that the average rainfall is appreciably higher at the elevation of 2,200 m. since this altitude is within the zone of cloud gathering. The vegetation was a luxuriant primeval forest, typical of this elevation.

The characteristic soil-profile showed the following sequence of horizons:

Horizon A_1 from 0–15 cm.—a moist, dark-brown humous layer, uniformly sandy in texture, loose, friable and porous, without stones or gravel, $pH = 5.5$

Horizon A_2 from 15–35 cm.—a moist, dark yellowish-brown layer of same texture and structure as A_1, $pH = 5.8$

Horizon B from 35–100 cm.—a moist, light yellowish-brown layer of same texture and structure as A_1 and A_2, $pH = 5.3$

Samples of each horizon were collected and analyzed under the serial numbers 101, 102 and 103 respectively.

In the air-dry condition, A_1 (101) has a dark chestnut-brown color, A_2 (102) a lighter shade of chestnut brown while B (103) has a distinctly reddish tint, which may be likened to chocolate brown.

The *mechanical analyses* as reported in Table 4 show all three horizons to be in the textural class of sands. There is little difference between A_1 and A_2, while B contains somewhat more of grade 4 (fine sand). The small amount of clay, < 0.005 mm., should be emphasized as well as the fact that it decreased downward, another anomalous fact to be spoken of later.

The *mineralogical analyses* show 101 to have about 7 per cent. (by weight) of heavy constituents of s.g. > 2.95, 102 about 2 per cent. and 103 about 1 per cent. The individual minerals are dominantly angular in shape, except for the secondary chert and flint and the shot-like concretions of iron oxides to be spoken of presently. The same minerals as present in the Lawou soil were found here, viz., plagioclase feldspars, some quartz, translucent chert or flint, hornblende, augite, hypersthene, magnetite, ilmenite (?) and pieces of tuff, more or less coated with iron oxide. Debris of plant structures and charcoal-like substances occupy the greatest bulk in 101 and 102, imparting a dark gray-black color to the A_1 horizon. This color is somewhat modified in A_2 by the presence, in about equal bulk as the organic matter, of opaque,

amorphous, shot-like, chocolate-brown concretionary grains of various sizes, which are the dominant constituents in the *B*-horizon. In the dry state the material is brittle, can easily be crushed, is homogeneous and has a specific gravity of 2.56. It represents undoubtedly a gel-mineral of iron hydroxide, more porous than limonite and hence of lower specific gravity

TABLE 6

Java (Neth. East Indies): Mt. Malabar; S.S.W. slope at 2,200 m.

CHEMICAL ANALYSES

Ss means determined by author;
So means determined by B. A. Soule;
Columns I refer to the percentages in the air-dry sample as is;
Columns II refer to the same percentages recalculated on the basis of the mineral part = 100.

	Serial No. 101 Horizon A_1 0–15 cm.	Serial No. 102 Horizon A_2 15–35 cm.	Serial No. 103 Horizon B 50–70 cm.
Ss *pH*	5.5	5.8	5.3
" Moisture 100°	6.30	7.22	8.20
" Ignition loss	38.29	32.87	30.67
" "Black matter" (alkali-soluble humus)	13.15	13.00	7.45

TOTAL ANALYSIS

	I	II	I	II	I	II
So Ignition loss	43.95	—	34.00	—	40.80	—
" SiO_2	26.91	48.01	30.60	46.37	19.90	33.61
" TiO_2	0.90	1.61	0.98	1.49	1.26	2.13
" Al_2O_3	12.23	21.82	15.50	23.49	21.98	37.13
" Fe_2O_3	7.07	12.61	10.75	16.26	11.80	19.93
" MnO	0.12	0.21	0.15	0.23	0.14	0.24
" CaO	4.46	7.96	3.59	5.44	0.82	1.39
" MgO	2.10	3.75	2.02	3.06	1.45	2.45
" Na_2O	1.11	1.98	1.01	1.53	Tr.	—
" K_2O	0.50	0.89	0.50	0.76	0.18	0.30
" P_2O_5	0.25	0.45	0.34	0.52	0.29	0.49
" SO_3	0.45	0.80	0.38	0.58	0.98	1.66
	100.05		99.80		99.60	
" Total carbon	16.68		13.26		7.81	
" Organic matter (C × 1.724)	28.76		22.81		13.46	
" Fe_2O_3 (colloidal)	1.42	2.53	Tr.	—	7.04	11.89
id. *id.* (in total Fe_2O_3)		20.08		—		59.66

because of its granular structure. These characteristic grains were not present in the original soil under natural conditions; they could not have been overlooked had they been present.

The *chemical analyses* as tabulated in Table 6 will be discussed with the next soil, viz.,

Soil from Mt. Banahao, Luzon

Only one soil from the Philippines was found suitable for our purpose. It was collected February 15, 1926, on the S.W. slope of Mt. Banahao, Luzon (about 14° N.), at an elevation of 2,100 m.

Climatic records are available for one of its tops on the N.E. side at an altitude of about 2,100 m. The records were collected by Dr. W. H. Brown of the Philippine Bureau of Science for the period of one year, from November 1915 to November 1916, and show a mean temperature of 14.6° C. (58° F.) and a mean annual rainfall of 7,468 mm. (294 inches).[23] Brown remarks as follows on the monthly distribution of rainfall: "the rainfall on the northern and northeastern slopes of Mt. Banahao is distributed throughout all the months of the year, and there are no distinct wet and dry seasons." Practically the same conditions may be assumed to prevail in our locality.

Mt. Banahao, like Mt. Malabar (and Mt. Lawou) in Java, is an extinct strato-volcano of the Vesuvian type, although it is true that the writer found more basaltic lava at its base than he has met with in Java. Only one eruption of the year 1730 is known to have taken place in historical times.[24] But like the Javanese volcanoes, Mt. Banahao has received andesitic pyroclastics from nearby volcanoes, notably from Mt. Taal, located some 45 km. to the west. During its explosive eruption of January 27, 1911, Mt. Taal, according to Pratt, covered an area of 2,000 square km. with ash, which was deposited to a distance of 52 km. from the crater, that is, in a radius which includes Mt. Banahao. This is only one of a number of eruptions which Taal volcano has had all during recent and remote historical times.

The soil-forming material on Mt. Banahao shows the same origin from andesitic pyroclastics as does the parent-material of the Javanese soils, mentioned above.

The typical soil profile at an elevation of 2,100 m. under tropical mountain forest was as follows:

Horizon *A* from 0–20 cm.—a moist grayish-black humous layer, uniformly sandy in texture, loose and friable, $pH = 5.8$

Horizon B_1 from 20–25 cm.—a thin layer, peculiarly turkish red or orange red in color; sandy texture, friable, porous, without concretions, stones or gravel; $pH = 4.9$

Horizon B_2 from 25–100 cm.—a moist yellowish-brown sand, friable, with decomposed parts of the original pyroclastics up to pebble size, thoroughly weathered and easily crumbled between the fingers; $pH = 6.1$

Samples were taken and analyzed of each horizon separately under the serial numbers 121, 122, 123 respectively.

In the air-dry condition, horizon *A* (121) shows a dark chestnut-brown color, horizon B_1 a lighter chestnut brown with brick-red specks and horizon B_2 a somewhat lighter shade than B_1 with the same orange-yellow or brick-red specks. Numbers 122 and 123 particularly had to be broken up in a mortar with rubber-tipped pestle since the samples had become hard and lumpy. Practically all of the material passed through the 2 mm. sieve.

The results of the *mechanical analyses* are tabulated in Table 4. The whole soil is a sand according to the classification of the United States Bureau of Soils. Numbers 122 and 123 resemble one another more closely than they do number 121, a fact that is borne out by the chemical analysis. The decrease downward of clay particles < 0.005 mm. may again be emphasized in this case as representing quite the opposite from the conditions in an ordinary soil which usually increases in clay-content with depth.

The *mineralogical analyses* show about 4 per cent. (by weight) of the heavy constituents in the topsoil and 8 per cent. in the subsoil. No individual minerals other than those men-

tioned in the Malabar soil were found here, thus suggesting a parent-material of about the same composition in both cases. Organic matter is as dominant in number 121 as it is in 101, imparting a dark grayish-black color to all grades. While organic matter is still present in number 122 (as also in 123), yet its color is overshadowed by orange-red concretions of all sizes from 2 mm. down, the individual grains having a less regular shape than those of numbers 102 and 103, described previously. They are brittle in the dry state, and are homogeneous in composition and not merely coatings on mineral particles. They doubtless represent a similar gel-mineral of iron oxide, even if the color is somewhat different. It has namely been shown by colloid-chemists that ferric hydroxide gels can be made to exhibit colors ranging from yellow to red by merely increasing the size of the colloidal micellæ. In this way red forms have been produced from finely divided yellow particles by agglomeration.[25]

Discussion of the Chemical Analyses

It was soon realized in the course of the laboratory investigations that neither the mechanical nor the mineralogical analysis, nor both together, could lead to quantitative results which would enable one to draw conclusions concerning the relation between climate and the particular form of weathering and soil-formation encountered in tropical high altitudes.

The chemical analysis was therefore resorted to for possible clues.

Although a great many analyses of Javanese and Philippine soils are known (see for instance *op. cit.*, 20, and bibliography number 25), yet they are of little or no value for our purposes. They were made for agricultural purposes according to the methods of partial analysis in use in agricultural chemistry, so that they often contain only determinations of the agriculturally important elements, K, P, N, Ca, besides organic matter. Furthermore, no elevations are mentioned because no interest had been shown as yet for the soils of the high-mountain regions which only concern us here.

It was therefore necessary to have complete analyses specially made, the number of which being largely determined by financial carrying power. This was also the most cogent reason for limiting the present study to the particular phase of weathering in high altitudes.

The results of the chemical analyses are tabulated in Tables 6 and 7.

TABLE 7

Luzon (Philippine Islands): Mt. Banahao; S.W. slope at 2,100 m.

CHEMICAL ANALYSES

Ss means determined by author;
Sh means determined by Earl V. Shannon;
Columns I refer to the percentages in the air-dry material as is;
Columns II refer to the same percentages recalculated on the basis of mineral part = 100.

	Serial No. 121 Horizon *A* 0–15 cm.	Serial No. 122 Horizon B_1 20–25 cm.	Serial No. 123 Horizon B_2 25–50 cm.
Ss *pH*	5.8	4.9	6.1
" Moisture 100°	8.40	11.18	10.95
" Ignition loss	38.04	30.45	27.23
" "Black matter" (alkali-soluble humus)	13.91	n.d.	6.97

TOTAL ANALYSIS

	I	II	I	II	I	II
Sh H_2O – 110°	8.76	—	11.60	—	11.60	—
" H_2O 110°	10.32	—	12.20	—	11.97	—
" Ignition loss (in excess of H_2O)	29.26	—	16.93	—	14.61	—
" SiO_2	21.26	41.15	22.37	37.74	22.08	35.72
" TiO_2	0.70	1.39	0.64	1.08	0.87	1.41
" Al_2O_3	15.35	29.71	21.82	36.83	22.62	36.59
" Fe_2O_3	5.74	11.11	8.79	14.83	9.83	15.90
" MnO	0.08	0.15	0.06	0.10	0.06	0.10
" CaO	3.48	6.74	2.47	4.17	2.52	4.08
" MgO	1.76	3.41	1.96	3.31	1.93	3.12
" Na_2O	1.73	3.36	0.52	0.87	1.26	2.04
" K_2O	1.39	2.69	0.36	0.61	0.36	0.58
" P_2O_5	0.33	0.64	0.25	0.42	0.22	0.16
" S	0.17	0.33	0.18	0.30	0.10	0.26
	100.33		100.15		100.09	
" Organic matter ($CO_2 \times 0.471 \times 2$)	30.50		17.49		14.88	
" Fe_2O_3 (colloidal)	4.23	8.19	6.95	11.73	8.90	14.40
id. *id.* (in total Fe_2O_3)		79.69		79.07		90.54

Their examination reveals the fact that soils 101 and 102 are practically the same and so are soils 122 and 123. It is therefore considered justifiable to confine the discussion mainly to the different pairs 101 and 103, respectively 121 and 123, as representing the *A* and *B* horizons of the Malabar and Banahao soils. Numbers 102 and 122 will only occasionally be referred to.

The minor constituents, such as MnO, TiO_2, P_2O_5 and S, do not materially affect our problem, so that they will likewise be omitted from the discussion; their determination was mainly carried out for completeness' sake.

The following points require our greatest attention as bearing directly on the problem under investigation:

(1) the soil reaction as expressed in the approximate hydrogen-ion concentration determined in the field;

(2) the distribution of organic matter, both total and soluble (black matter);

(3) the distribution over the soil profile of the alkali and alkali-earth metals;

(4) the distribution over the soil profile and the migration of SiO_2;

(5) the distribution over the soil profile and the migration of Al_2O_3;

(6) the distribution over the soil profile and the migration of Fe_2O_3.

An examination of the analytical results, as recalculated on the basis of the mineral part = 100, reveals that:

Ad. 1—both soils are acid over the entire profile, medium to strongly acid as regards the intensity of acidity expressed in the hydrogen-ion concentration;

Ad. 2—the humus content decreases downward in the normal way, but even the lowest horizon contains abundant humus, viz., the kind of acid or "raw" humus such as is found in peat-bogs;

Ad. 3—the alkalis and alkali earths are richer on top than in lower horizons, an anomalous condition that has previously been pointed out and explained;

Ad. 4—SiO_2 in both soils increases upward, proportionally more in the Malabar soil than in the Banahao soil;
Ad. 5—Al_2O_3 increases downward, proportionally more in the Malabar than in the Banahao soil;
Ad. 6—Fe_2O_3 increases downward proportionally more in the Malabar than in the Banahao soil.

It has been stated before and may be repeated here, that the process of podsolization results in a (relative) increase of silica in the upper horizon and a decrease of alumina and iron oxides, brought about by the presence of acid humus in the water percolating through the soil-forming material, thereby causing the soil to be bleached. O. Tamm, who has recently made a thorough investigation of the problem, states that no kaolinization, *i.e.*, clay-formation, occurs in this process.[28]

The opposite of podsolization is laterization, which results in a decrease of silica in the upper horizon and an increase of alumina and iron oxides due to the absence of humus in the percolating water.

Thus the results of the chemical analyses indicate that podsolization rather than laterization takes place in the Malabar and Banahao soils.

The mechanical analyses have previously established the fact that clay is practically absent, which is in accordance with Tamm's findings and points toward the occurrence of podsolization.

Finally, the field evidence has shown that the A_2 horizons are distinctly lighter in color than either the A_1 or the B horizons.

Hence we may reach the conclusion that:

Podsolization does take place in high mountain zones of the Oriental tropics in much the same way as it does in the lowlands of the middle latitudes.

Differences do exist, but these differences are such of degree only and not of fundamental character.

Thus the bleached horizon is often much thicker in the tropical high mountain zones than is ordinarily the case in the middle latitudes. This fact indicates a greater intensity in

the process of podsolization, which is readily accounted for by considering that the rainfall is much greater in the tropical high mountain zone than in the middle latitudes, where podsolization usually ocurs. Thus the leaching and bleaching processes are naturally bound to be more intensive.

Nevertheless, it might be argued that a true podsol, which consists of little else but silica, has not been found anywhere. This objection also finds a ready and natural explanation when we consider the genesis of the soil-forming parent-material and its episodic additions of fresh pyroclastics which take place to this day as they have taken place in the near past.

Thus the results of these investigations lend good support, from the Oriental tropics, to the principles upon which is built the modern climatic classification of soils, and the author hopes for a chance to verify these findings in the Occidental tropics.

At this stage it is fitting to inquire into the mechanism of the migration of silica, alumina and iron oxides as these are the main constituents the removal or enrichment of which is characteristic of the process of podsolization.

As for *silica*, it was thought, at one time, that in chemical weathering silica was transported in molecular solution as a silicate. Nowadays the consensus of opinion is that silica migrates mostly as a colloidal solution, as a hydrosol of SiO_2 the electrical charge of which may be negative or positive. According to Taylor [28] hydrous silica is positively charged in acid solution. Consequently it is so charged in our acid soils. In this condition it is very unstable and easily precipitated as an irreversible gel by an electrolyte which neutralizes its charge. When so precipitated silica becomes more stable and resistant to solution and subsequent transportation. The chemical analyses show that our soils contain large amounts of alkali and alkali-earth compounds in the upper horizon, which upon weathering give rise to negatively charged electrolytes, viz., the bases such as $Ca(OH)_2$, $Mg(OH)_2$, KOH, NaOH. Thus it is easily seen that favorable conditions are present for the silica hydrosol to be precipitated and kept in the upper

horizon by interaction with the liberated bases. Since the base-liberating substances decrease downward, gelation and insolubilization of the silica will also decrease downward, and relatively more silica hydrosol can be removed to still lower depths, thus accounting for the decrease of silica downward.

A confirmation of this view may be found in the fact, noted above, that the silica has proportionally decreased more in the lower horizon of the Malabar soil than of the Banahao soil. This fact can be readily explained by comparing the sums of the alkali and alkali-earth metals in both soils. Such a comparison shows that the Malabar soil has retained less of the base-forming elements than the Banahao soil. The figures are 14.58 per cent. and 4.14 per cent. respectively for the *A*- and *B*-horizons of the Malabar soil, 15.20 per cent. and 10.82 per cent. for the corresponding horizons of the Banahao soil. Thus it is readily seen why the downward migration of silica has taken place more slowly in the Banahao soil than in the Malabar soil.

A study of the migration of *alumina* is capable of correcting an error in the minds of many geologists and petrologists concerning the stability of aluminum in rock-weathering. The general assumption is that in the weathering of rocks aluminum oxide is the most stable component of minerals. For that reason chemical calculations of alterations are based on the assumed immutability of alumina. This assumption was perhaps founded on the fact that aluminum does not form readily soluble and removable compounds, carbonates for instance, such as iron, potassium, calcium, etc., do.

Soil students have known for some time that aluminum oxide, as it occurs in soil-forming mineral matter, is subject to rather ready removal in the presence of organic matter. The chemical analyses of our two soils are illustrative of this fact. Caution should therefore be exercised in the interpretation of chemical analyses of weathered rocks recalculated on the basis of alumina as a fixed standard, unless the conditions of weathering are stated and exclude the possibility of the presence of appreciable quantities of organic matter.

It is now generally agreed that aluminum is transported, or migrates, as a hydrosol of the probable composition $Al(OH)_3$ or $Al_2O_3 . 3H_2O$ with a positive charge. In inorganic solutions it is easily precipitated by electrolytes of opposite charge, but in humus-laden water the humus acts as a protective colloid and can prevent altogether or simply delay the coagulation of $Al(OH)_3$ in the presence of electrolytes or of other colloids with opposite charge. We have noted the fact that our soils contain abundant humus all the way down. Thus the conditions are present for most effective leaching downward of alumina in our soils, as is indicated by the chemical analyses.

It is true, however, that Aarnio, in an exhaustive study of the influence of humus-laden water on the formation of podsols in Finland (as recorded by Stremme [29]), has demonstrated the fact that it requires appreciably more humus to prevent the coagulation of Al_2O_3 sols than it does to prevent the precipitation of Fe_2O_3 sols. Hence, under the same conditions for both, relatively less Al_2O_3 will be carried to greater depths than Fe_2O_3. This fact is similarly indicated in the chemical analyses of our soils.

The greater amounts of alkali and alkali metals in the Banahao soil, as compared with the Malabar soil, easily accounts for the fact, noted before, that relatively less alumina has migrated downward in the Banahao soil than in the Malabar soil.

The field-criteria for determining whether podsolization takes place, are based on the changes in soil-colors due to the removal of iron oxides as the principal coloring matter. It is therefore justified to inquire more fully into the causes and mechanism of *iron oxide* migrations under the influence of humus-laden water.

The technical literature in various branches of science is full of theories that have attempted to explain the phenomena. Their great number suggests the importance that is attached to the problem from the side of geologists, petrologists, stratigraphers, soil-scientists, chemists, agronomists, engineers, etc.

The various theories may be conveniently grouped into the following three groups:

(1) Theories based on molecular reactions,

(2) Theories based on colloidal reactions,

(3) Theories based on the action of micro-organisms.

Ad. 1.—According to Stremme (*op. cit.*, 17*a*, pp. 133–134) the German F. Senft was the first to clearly point out (1876) the molecular-chemical mechanism of the removal of iron by bog-waters, *i.e.*, by waters laden with organic matter or acid humus such as is the case in our soils. The following passage may here be quoted from Senft's book on "Rocks and Soils" (Munich, 1876):

> "If weathering of these minerals (feldspars, micas, hornblende) takes place under the exclusion of air, or under the unhampered influence of water, laden with carbon dioxide or ammonia—as is namely the case when a rock surface has a thick cover of decaying vegetable matter—then all ferro-iron in feldspars, micas and hornblende is changed to iron bicarbonate or to ferroammonium compounds and is leached out as such by percolating water, as a result of which the clay-in-the-making is freed from all admixtures of iron oxide and of the ochre-yellow coloration it imparts, so that the clay finally becomes white in color. Upon further leaching of the alkali and alkali-earth metals this clay becomes kaolin."

This theory, modified as well as unmodified, has been adhered to by a great many prominent geologists and soil-students, such as Emeis, van Hise, Ramann. It is still being advocated by some, such as Stremme and Stahl in connection with the cause of the white color of kaolins and by Berg in connection with the origin of sedimentary iron-ore deposits.[30]

Ad. 2.—The colloid theory of iron-migration, perhaps, originated some thirty years ago with the Dutch physical-chemist van Bemmelen, whose name will always be linked with the first systematic treatment of weathering processes as colloid-chemical phenomena. But according to Stremme again [29] the Russian agricultural chemist Gedroiz was the first (1908) to let colloid chemistry bear upon soil-formation proper with special reference to Fe, Al and Si.[31] The three

elements start their migration as hydrosols of Fe_2O_3, Al_2O_3 and SiO_2 respectively. These are fairly unstable and are easily precipitated as irreversible gels in the absence of organic matter. But when organic matter is present, which acts as a protective colloid, they may remain a long time as sols and thus be carried for great distances by the percolating water. These facts have been amply corroborated by subsequent workers, conversant with the teachings and methods of modern colloid chemistry.

Ad. 3.—According to Ransome it has been known since 1836 that certain bacteria have the power of withdrawing iron from solution and causing its precipitation as ferric hydroxide. A great many contributions have since been made to the knowledge of the action of organisms in the processes of transformation, migration and deposition of iron. These contributions have not only been made by biologists, but by geologists as well. E. C. Harder has recently given an able summary of this and related subjects to which reference may be made.[32]

The writer confesses a preference for the colloid theories for reasons that need not be detailed here.

In the light of these theories, which have been amply substantiated by facts, the downward migration of iron in our particular soils is readily explained as being due, again, to the abundance of organic matter at all depths. This organic matter, acting as a protective colloid, enables the ferric hydroxide to remain a long time as a hydrosol, so that it can be carried downward for great distances in the percolating soil water before it is precipitated as an irreversible gel.

The question might be raised why the ferric hydroxide sols have not been gelated anywhere within visible depth to form a distinct layer of iron "hardpan" or iron-concretions, such as is often the case in true podsols of the middle latitudes.

Anticipating the explanation, it may be suggested that, again, the abundance of organic matter at all depths has prevented the iron hydroxide from being precipitated in any one layer within the range of depths studied in the field.

Modern colloid chemistry teaches [33] that gelation of the hydrosol of an irreversible or lyophobic colloid, such as is colloidal ferric hydroxide, can take place only in two ways:

(1) by neutralization of the charge,
(2) by dehydration.

Ad. 1.—Complete neutralization of the charge is not likely to occur down to the investigated depths because of the large amounts of protective colloids all the way down. The amount of black matter or soluble humus is particularly significant in this respect; it is quite appreciable even at the lowest depth. Besides, the amount of bases has greatly decreased in the lower horizons, and hence will be still less effective as a possible flocculating agent.

A special case of flocculation by neutralization of the electrical charge is perhaps the following phenomenon, which has been intensively studied by Sahlbom.[34] When a strip of filter paper is hung into an aqueous solution of ferric hydroxide sol the water will rise to great height, but the ferric hydroxide is precipitated at the base and remains there. Sahlbom found that this generally happens with positive colloids flowing through capillary tubes of 0.15 mm. maximum diameter, and is independent of the chemical nature of the capillary medium. The phenomenon is due to the generation of electromotive forces by the flow of the dispersive medium in the capillaries. These electromotive forces bring about a discharge or neutralization of the positively charged colloidal particles and consequent precipitation.

It might be argued that this phenomenon should have given rise to "iron hardpans" in our soils. Even if the mechanical analysis show them to be sands, it is conceivable that the capillary openings between the individual soil-particles are less than 0.15 mm. in diameter, thus fulfilling the necessary conditions for the precipitation of the ferric oxide sols.

The fact is, however, that such a precipitation has not taken place and no "hardpans" were found anywhere to the

investigated depths. The explanation of this fact lies in the acidity of the soil-reaction. Sahlbom has namely found, also, that the addition of an acid caused the ferric oxide sol to rise in the capillary columns of the filter paper. This is partly due to the peptization of the precipitated gel and partly to a rise in the precipitation potential of the colloid. Thus the medium to strong acidity of our soils can well account for the lack of hardpan formation.

Ad. 2.—Dehydration is likewise improbable as a possible cause for the gelation of iron hydroxide sols in our soils because they are constantly moist to wet all the year around. They were found to be in that state even during a period of exceptional drought, as has been noted before. This circumstance, together with the prevailing low temperature all the year round, eliminates dehydration as a cause for the development of an "iron hardpan" or of iron oxide concretions in situ.

However, it has been pointed out in the discussion of the mineralogical analyses that reddish-brown grains of some partially hydrated iron oxide were found to prevail in the *B*-horizons of both soils. They were not found in the soils in situ. In consideration of the high contents of easily soluble iron oxide, as per the Demolon extraction, it is held that the grains have developed subsequently. In other words, owing to the unnatural desiccation to which the soils have been subjected once they were removed from their natural environment, the ferric hydroxide sols in situ have become gelated, by partial dehydration, to form the grains just spoken of.

This is a process to which van Bemmelen has ascribed the dehydration of hydrated ferric oxide under the natural conditions prevailing in hot tropical lowlands where laterites are developed.

Shaking for 24 hours in the ammoniated water used as a preliminary to the mechanical analysis, according to the United States Bureau of Soils method, did not seem to bring about the deflocculation of those grains. They may thus have masked the real texture of the soil.

These considerations should therefore caution one in the

interpretation of mechanical analyses of soils which contain large amounts of organic matter and ferric hydroxide, unless their original moisture condition has been preserved. The author is not aware that this point has ever been considered before, so that it may not be amiss to emphasize it here.

Summary and Conclusions

1. Field and laboratory studies were made to test the validity of the principles of the modern climatic classification of soils.

2. Specifically, the object was to ascertain whether podsols are found, respectively whether podsolization takes place in tropical high altitudes, as the principles demand.

3. The field observations were made in the Oriental tropics.

4. The laboratory investigations were carried out on soil-profiles collected above 2,000 m. on the islands of Java (N. E. I.) and Luzon (P. I.).

5. Field and laboratory studies indicate that podsols are not found in the high altitudes of the Oriental tropics, but that the process of podsolization does occur in much the same way as it occurs in the middle latitudes.

6. Minor differences of degree are pointed out and naturally accounted for.

7. The evidence lends support to the modern classification of soils as based on the climatic control of rock-weathering and soil-formation.

8. The mechanism of podsolization is discussed with special reference to the investigated soils.

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THE CHANGED CONCEPTION OF THE CONSTITUTION

By JAMES M. BECK

(*Read March 26, 1930*)

Mr. President and Fellow-members:

I avail myself of this occasion to express the great pleasure, which my election several years ago to the fellowship of this venerable and historic Society gave me. Few honors, if any, that have come to me in my modest career have given me greater satisfaction. I have been a lifelong admirer of its illustrious founder, Dr. Franklin. In my judgment, he was not only the greatest intellectual genius of the eighteenth century, but I would rank him with the few supremely gifted and myriad-minded intellects of all time. In the Elysian Fields, his place is with Leonardo da Vinci, Michelangelo and Francis Bacon. To be a member of a society founded by Benjamin Franklin is an honor that any man may reasonably covet.

In addressing myself to my theme, "The Changed Conception of the Constitution," I may say, as the fair Desdemona on a famous occasion, "I do perceive here a divided duty." For a half hour I am privileged to address an invisible audience, possibly numbering millions, in a miraculous manner, of which even the unequalled vision of a Franklin would never have dreamed, and simultaneously I speak to a visible audience, which, while it expects me to speak a longer time, yet may feel after I have concluded that it needed less time to demonstrate how little I know about the Constitution. It is a rare privilege to hitch one's wagon to the star of the radio, but the radio, like time and tide, wait for no man and before I shall complete my full address, I shall be suddenly whirled from a million different directions throughout this broad land, where I had been talking to equal millions, back to my visible audience in the hall of the Philosophical Society. I only hope

that this will not occur at some point of my address, which will leave both my audience and myself "in the air," instead of "on the air."

I shall claim your attention tonight to a very brief discussion of the changed conception of our Constitution. The subject is so broad and admits of such variety in treatment that, like "a man to double business bound, I stand in pause where I should first begin." The idea of the address was suggested to me when I took occasion some time ago to restudy the great controversy, which agitated this nation a century ago and brought it to the very brink of dissolution. It culminated a century ago last January in the greatest intellectual duel that ever took place in the Congress of the United States.

No crisis in our national life ever came so suddenly. Only four years before the nation had celebrated the 50th Anniversary of the Declaration of Independence under circumstances of such general goodwill that the period was called the "era of good feeling." The community of patriotic interest, which then prevailed, had its pathetic and striking culmination in the fact that, on that 4th day of July, 1826, the two great protagonists of the first two political parties of the nation, Thomas Jefferson and John Adams, joined the silent majority.

The nation had known, in its earlier years, the most bitter and rancorous dissensions between the two political parties, which then divided the electorate, but one of them had disappeared and, in 1826, the American people seemed united as they had never been before and, I might add, since. Within a year the baleful spirit of sectionalism arose like a cloud no bigger than a man's hand, when the demands of the manufacturing interests of the North for a higher tariff clashed with the agricultural interests of the Southern states, and within the following year, John C. Calhoun published anonymously his famous "South Carolina Exposition," in which he asserted the right of a state to nullify any Federal law which the state deemed to be in violation of the Constitution. When Senator Foot, of Connecticut, a year later introduced his resolution to restrict the sales of the public lands, the West, then in its

infancy, accepted it as a challenge to the continued growth of the young states west of the Alleghanies and, as a result, an attempt was made to form a coalition between the South and the West against the East.

The debate on the Foot Resolution lasted for over four months and was the most notable debate that ever took place in Congress. It reached its climax on January 25–27, 1830, when Robert Y. Hayne, of South Carolina, an orator of exceptional ability, delivered in behalf of the coalition, his famous speech, in which, after attacking the political sincerity of the champion of the North, Daniel Webster, and after preferring a terrific indictment against the Northern states for alleged hostility to the South and the West, he concluded his argument by an elaborate exposition of the Calhoun doctrine, that any state, if aggrieved by any act of the Federal Government, could "interpose" and forbid the execution of the law within its own borders. This right was claimed under the Constitution and it was asserted that, in such an event, the only resource of the Federal Government was to propose an amendment, which, if ratified by three-fourths of the states, would then, and then only, sanction the disputed assertion of authority.

The next morning Daniel Webster, then in the full maturity of his extraordinary powers, arose to reply. Every one realized that a crisis in the life of the Nation had arisen. The reply was a masterpiece of forensic oratory. Ordinarily, the spoken word is like a quickly-burning fire, soon reduced to glowing embers and then to cold, white ashes. To reread a speech is to subject it to the disadvantage that it lacks the three things, which Webster once said made a speech truly eloquent, namely, the man, the subject and the occasion. Wanting these, the Philippics of Demosthenes and the great orations of Cicero are today generally dull reading, as are the debates between Burke and Fox, or Gladstone and Disraeli. Only an occasional passage retains any of the warmth of the encounter. The noble philosophy and the glowing rhetoric of Edmund Burke somewhat redeem his printed orations from

dullness, just as the vitriolic ferocity of Junius gives a languid interest in the political controversies of his time.

It is not so with the rereading of the Webster-Hayne debate. The fire of the contest still burns. One feels the impact of the mighty shields and the sparks still fly from the Damascene swords of the two duelists. Of Hayne's speech, it is high praise to say that it was, in all respects, worthy of the great reply. I regard Webster's speech as the noblest forensic effort in the English language. Inspired portions of it are Miltonic in their splendor. While it did not end the crisis, it developed what was then lacking, a militant sentiment for the Union, and the repercussion of Webster's masterpiece was felt when Lincoln issued, a generation later, his call to arms to defend the integrity of the Union.

The crisis, however, was ended for the time being when, a few months later, President Jackson, himself a Southerner, arose at a dinner in honor of Thomas Jefferson, and, looking meaningly at John C. Calhoun, gave the immortal toast, "The Federal Union—it must be preserved!"

However, it is not my purpose to narrate the story of this great debate, but rather to call your attention to the significant fact that none of the speakers, including Hayne and Webster, had any conception of the Constitution as a living organism. All regarded it as a static instrument, whose letter was as unyielding as the stone upon which the Ten Commandments were graven. It was the fundamentalist conception that the Constitution had a fixed and unchangeable meaning and that all questions of governmental power could be determined by answering the simple question: "Is it so nominated in the bond?" The conception of a Constitution as a living organism, susceptible of adaptation and slow development and itself controlled by an environment of unwritten constitutional principles, did not find any expression in these notable addresses. In discussing the question of nullification, Webster and Hayne confined themselves to the letter of the Constitution and wholly ignored the economic developments, which so profoundly affect all human institutions, whether

written or not, and which were destined to answer the Calhoun dogma of nullification by the centripetal influences of steam and electricity.

The modernistic interpretation of the Constitution, which sees in it only a partial expression of a great body of institutional law, which was to be characterized a generation later by Seward, as the "Higher Law," did not occur to either Webster or Hayne. It must be remembered that none of the disputants in this prolonged debate had any knowledge of the proceedings in the Constitutional Convention of 1787, for Madison, still living, was jealously guarding his invaluable record of the debates and none of that generation had any but the most fragmentary knowledge of what had taken place during those four months when the Convention discussed in secret session the nature of the Constitution. Thus, in the absence of any living interpretation of the Constitution, the disputants were naturally restricted to the text, as interpreted by the few pioneer decisions of the Supreme Court, and they argued the question as though the Constitution was a contract or bond, as such the sole expression of the American people as to their form of government, beyond which there could be no departure save by the slow process of a constitutional amendment.

The economic interpretation of the Constitution was equally beyond their ken, for the pastoral-agricultural phase of society was only then ending and the dawn of the modern mechanical era imperceptibly beginning with the development of the railroad. Indeed, Hayne, who afterwards became a pioneer in railroad construction, himself thus unwittingly helped to defeat his doctrine of nullification, for it may be questioned whether the Union would have been held together if the railroad had not welded the states by links of steel. They little realized that the meaning of the Constitution would within a generation be profoundly modified by the invincible influences of steam and electricity.

It is true that this changed conception of the Constitution, due to mechanical forces, has found even to this day little

open expression from the bench or bar. To judges and lawyers, the Constitution is as the Bible to the devout—infallible and omnipotent. The law is to Bench and Bar a religion; the judges are the high priests and the lawyers the inferior clergy. Both regard the Constitution in the spirit of unreasoning pietism. This is the more natural as the bar was originally the child of the Church and has never wholly escaped from that spirit of sacerdotalism, when the subtlety of the scholiast most prevailed. Thus, lawyers are too apt to regard the doctrines of the law as final truths, having their sanction in some judicial *ipse dixit*, or political document. They forget that, even in the field of religion, the Master said of the institution of the Sabbath, that it was made for man and not man for the Sabbath, and similarly, lawyers might well reason, considering the fallibility of all human laws, even of constitutions, that law is only the reasoned adjustment of human relations and, as these relations are forever changing, sometimes with kaleidoscopic swiftness, it follows that the institutions of the law can never be wholly static.

Even if legal conceptions could be accepted as final truths, yet it is impossible to define them in the imperfect medium of language with any finality, for the very meaning of words, even the words of the Constitution, changes from generation to generation and often the definition survives the text. This sacerdotal conception of law has led to much foolish expression about the sanctity of laws, whether they be wise or unwise, and we forget the elemental fact that we cannot ask a people to respect a law that is not intrinsically worthy of respect.

Another error of thought, which still prevails to a large extent, has caused this misconception of the true nature of the Constitution. We forget that there was an American nation, with its well-defined institutions, before either the Constitution or the Declaration of Independence. The American Commonwealth began with the landing of the first English settlers upon the coasts of Virginia, and the Commonwealth of the Colonies, even though it lacked organic existence, had its habits, customs and institutions, like those of the English

Constitution, which no written document could wholly supersede and of which the American Constitution was only a partial expression. For example, if the first ten amendments of the Constitution had never been adopted, they would, none the less, have been a part of our institutional law and the Constitution would have been interpreted to include these principles of Anglo-Saxon liberty. The Constitution simply gave them an added and higher sanction. Moreover, many of the expressions in the Constitution are not self-defining, but are simply the suggestion of a great body of unwritten law, which is thus written into the Constitution. For example, the guaranty of "due process of law" in itself means nothing, but it means a volume when interpreted in the light of the great principles of English liberty for a thousand years, which define what "due process of law" is.

It seems to me that we would have a larger and nobler conception of the Constitution if we regarded the Declaration of Independence and the formulation of the Constitution as very noble but, nevertheless, intermediate chapters in the great epic of the American Commonwealth. If Canada were to terminate tomorrow the slight remaining tie, which binds it to England, its career as a nation would not then begin. It has been, in truth, a nation from the first English settlement, and similarly, the American Commonwealth has been a nation from the time that the Virginia adventurers created that first-born of the Mother Parliament, the Virginia Legislature at Jamestown.

The Constitution is something more than a written and definitive contract. It is a living organism, susceptible of adaptation and, therefore, of increasing growth, and its vitality depends upon its correspondence with the necessities and spiritual tendencies of the American people. This only illustrates afresh the immortal truth of Aristotle, that any constitution which does not thus correspond to the "ethos" of the people will necessarily perish. While some learned Justices of the Supreme Court, in the true spirit of sacerdotalism, have affirmed that the Constitution today, means exactly what its

framers meant, yet no one can read the court's interpretations of the Constitution, contained in two hundred and eighty volumes of the Supreme Court reports, without being convinced that, with extraordinary ability, the Court has developed and adapted the Constitution, as a quasi-Constitutional Convention in a restricted sense, to the changing needs of the most progressive nation in this most changing period of the world's history. Thus, it cannot be seriously contended that one of the greatest of the Federal powers, namely, the regulation of interstate and foreign commerce, means today what the framers meant when they vested this power in the Federal Government. To them the division of governmental power between interstate commerce and intrastate commerce was extraordinarily simple, while its attempted application to a country welded together by the railroad, steamship, telephone, telegraph and the radio, has required a judicial subtlety, that has made our dual system of government, in the economic sphere, one of the most intricately complex nations of the world. In this respect, the men who framed the Constitution would not recognize their handiwork today.

The profound impact of a mechanical civilization upon our written form of government was appreciated by the greatest of all Chief Justices, John Marshall, when he said, in one of the greatest of his opinions (McCulloch v. Maryland):

> "This provision is made in a constitution intended to endure for ages to come, and, consequently, to be *adapted* to the various crises of human affairs."

I quote this, lest I be accused of heresy in my suggestion that the Constitution is, by usage, judicial interpretation, political habits and unwritten political institutions, necessarily subject to unceasing adaptation to make it serve the necessities of the most changing nation in the world.

The thought of an ever changing Constitution is not, in all respects, a comforting one, for, if it be a living organism and have within itself the potency for development and growth, yet, like all living organisms, it then also has within it the seeds of degeneration and possibly death. Such a conception

of the Constitution challenges the thought of each living generation of Americans to the great question whether this living organism is to grow in wisdom, or perish in folly.

The Constitution is not today what it was fifty years ago, nor was it then what it was a half a century earlier, and it is safe to predict that it will not be fifty years from now what it is today. The eternal inquiry arises, "*quo vadis?*" Are we treading the downward path to Avernus, from which escape is so difficult, or are we ascending to new and nobler heights of constitutionalism? That should be the great question for every thoughtful American.

Time will not suffice to illustrate my meaning by suggesting the portentous changes to which the Constitution has been subjected. I can only indicate a few by a passing sentence and in indicating these, I do not mean to suggest that some of them may not be desirable, for some undoubtedly conform to the economic needs of the nation and to the democratic genius of the American people. The destruction of the electoral college, except as an empty form; the profound change in the representative system, due to the changed democratic ideal that a representative should think *with*, and not *for* his constituents; the breaking down of the barriers that once imperfectly marked the different functions of the Executive, Legislative and Judiciary; the steady deterioration in power of Congress, as the great Council of the Republic and the corresponding aggrandizement of the Executive, until he is, in everything but name, an elective King for a short tenure; the perversion of the taxing power, whereby the Federal Government assumes powers never granted to it; the even greater perversion of the power of appropriation, whereby the Federal Government has persuaded the states, by the moral bribery of Federal grants, to yield their reserved powers; the destruction of the equitable principle that direct taxation should be apportioned among the states in proportion to political power in the House of Representatives; the denial by the Senate of the right of the states to choose their own Senators, except by and with the advice and consent of the Senate; the denial of

the right of the states to determine, in respect to their local conditions, the qualifications of an elector; the slow destruction of the power of the state over domestic commerce by the expansion of the Federal commerce power; the creation of numerous bureaus and some departments to effectuate purposes, which are not within the sphere of Federal power; the socialistic experiment of aiding failing industries by grants from the Federal Treasury; the perversion of the taxing power to re-distribute wealth; the appointment of diplomatic representatives to represent our country in foreign lands without the sanction of the Senate; the power to declare war without the consent of Congress by acts which make war inevitable, and, finally, the crowning atrocity of the Eighteenth Amendment, which invades individual liberty in a manner, at which Washington and Franklin would have stood aghast and which, in this respect, relegates the once proudly conscious states to the ignominious position of being mere police provinces.

These are only a few illustrations of the profound changes which have been wrought in one hundred and forty-three years of constitutional development. As I have said, some of them may be advantageous, but certainly not all of them. Many of them constitute a revolutionary change in the conceptions of liberty, which were supposed to have been unalterably written into the Constitution.

Taking a single illustration: No principle of English liberty was more dear to our forbears than the idea that only the representatives of the people assembled in Congress could impose a tax. For that right our English forbears had gone to the scaffold and many of the great battles of English liberty were fought about this principle. We separated from the mother country upon this principle that direct taxes could only be imposed by the consent of the representatives of the people. To confirm this conception of liberty, the framers of the Constitution not only expressly provided that Congress, and not the Executive, should impose taxes, but that all revenue bills must originate in the House of Representatives, as the more directly representative body of Congress, and

yet the House of Representatives recently passed a law, which gave an almost unlimited discretion to the President, with the aid of the Tariff Commission, to raise or lower any duty to the extent of fifty per cent of the statutory rate. What does this mean in concrete terms? Every cent per pound that is levied upon the importation of sugar means a burden to the American people of approximately one hundred million dollars. Suppose the tax, as passed by the Congress, is three cents per pound. If the Flexible Tariff provision, as passed by the House, shall prevail at this session, the President can make the duty either four and a half cents, or one and a half cents, a difference of three cents a pound, and, therefore, either a diminution of the tax burden of one hundred and fifty million dollars, or an imposition of a like burden upon the consumer, and yet, when this provision was under consideration by the House, only a few of us could see that it involved, for better or worse, an abandonment of a time-honored principle of English liberty, and a palpable violation of the Constitution.

I appreciate that many of these changes are the inevitable results of the impact of a mechanical civilization. Take, for example, the more comprehensive question, which once agitated the American people, that of centralization. When the Constitution was adopted, the states had a very real consciousness of their own sovereignty, while the conception of national unity was a very slow growth. All this is changed in our lifetime as a result of a mechanical civilization. The old struggle against centralization has largely passed away. Only lip service is longer paid by either of the great parties to the rights of the states. The "indestructible union of indestructible states" is now little more than a rhetorical phrase. Each of the great political parties, when in power, vies with the other in consolidating the Union by multiplying bureaucratic agencies, for which no true authority can be found in the Federal Constitution.

To the extent that this is the result of economic forces, it is irresistible, even if not always desirable, but it is, in part, due to that greed for power, which grows by what it feeds

upon. Some of us believe that the Constitution cannot survive if the planetary system of the states be wholly absorbed in the central sun of the Federal Government. Our nation is too vast in area and our people too numerous to be governed altogether from Washington and yet it seems impossible to combat the tendency towards centralization, when this "ethos" of the people, of which Aristotle spoke, demands it. The portentous difference between the American people, when they framed the Constitution, and the American people today is this: our forbears thought in terms of abstract political rights, but we today think in terms of concrete economics. Moreover, the gospel of the American people today is efficiency and to secure such efficiency they are apparently willing to sacrifice any principle that makes for the greater consideration of security.

We can measure this in the contempt of the people for Congress and their confidence in the Executive, whoever he may temporarily be. In nearly every controversy between the Executive and the Congress, the people sympathize with the Executive, for they can visualize a single individual and make a legend of him, but the multi-headed Congress makes no appeal to their imagination. They share the relief of the President when he no longer has "Congress on his hands," to use the popular expression.

This, in itself, is an amazing change in the ethos of the people, for our Constitution was formed when the traditions of the great English Revolution of 1688 were still dominant in men's thoughts. Then the people were jealous of executive power and established in England the supremacy of Parliament. Today many Americans subconsciously believe that the United States would be better off, if the President were made a Committee of One for the Union. That this is their ethos is shown by the fact that, in our industrial development, all government of corporations tends to concentrate power and, therefore, responsibility, in one man, and we cannot think in terms of one-man power in industrial development without a reflex effect upon our conception of that larger corporation, which we call our government.

I confess I cannot see the way to combat this changed consciousness of the American people, which is so largely due to mechanical forces, which no written Constitution can overcome.

Indeed, our very dependence upon a written Constitution and our mistaken belief in its static nature and its self-executing powers has tended to deaden the political consciousness of the American people. They mistakenly believe that, in some way, the Constitution will save itself and they have the wholly illusory idea that, if Congress passes unconstitutional laws, the Supreme Court will, in some way, invalidate them and that, therefore, the people need have no concern about such invasions of the Constitution. The idea that the Supreme Court has unrestricted and plenary power to disregard and invalidate an unconstitutional statute is the great illusion. In the first place, the Supreme Court can invalidate no law unless it is involved in a concrete case and often no such case ever arises. In the second place, the Supreme Court, necessarily influenced by the spirit of democracy, sustains any doubtful statute unless its conflict with the Constitution be clear and almost indisputable.

Moreover, there are many laws, which involve questions of political discretion, which can never come before the Supreme Court, either because their invalidity depends upon the motive of the legislators, or because they involve profound questions of political discretion, upon which the Court is not competent to act. I do not minimize the great work of the Supreme Court and I realize that its conservative course is the only one it could pursue to retain its prestige, but when I tell you that less than fifty Federal statutes have been declared invalid by that court in the history of the government and that thousands of laws have been passed by Congress for which no possible warrant can be found in the Constitution, you will appreciate that the Court cannot, or at all events does not, in all instances defend the Constitution.

The conclusion is that the Constitution, as a living organism, is in process of deterioration, and not of growth. To

change the metaphor, it is much as the New Jersey beach at my summer home, and I here venture to quote what I said in this respect in my book, "The Vanishing Rights of the States":

> "The encroaching waves each day ebb and flow. At high tide there is less beach and at low tide more. At times the beach is devoured by the ocean, when a tempest has lashed it into a fury, and then the waters will become as placid as a mountain lake, and the shore will seem to have triumphed in this age-old struggle between land and water.
>
> The owner of the upland is often deceived by the belief that the fluctuations of the battle generally leave the shore line intact, but when he considers the results of years, and not of months, he will realize that the shore has gradually lost in the struggle and that slowly, but steadily, the ocean is eating into the land."

If we of today, engrossed as we are in the complexity of this modern-day world, fail to see how the upland of the Constitution is being slowly destroyed by the erosion of the waves of innovation, yet the men who framed the Constitution had no illusions as to its perpetuity. Thus, the founder of this society, after the Constitution was adopted, said, with his usual genial humor,

> "Our Constitution is in actual operation; everything appears to promise it will last, but in this world nothing is certain but death and taxes."

Indeed, on the last day of the Convention, when the aged Franklin—as some say, with tears in his eyes—implored the reluctant delegates to sign the great compact, which was to immortalize them all, and won their consent by his skillful and ingratiating speech, he made this prediction:

> "There is no form of government but what may be a blessing to the people if well administered for a course of years, and can only end in despotism, as other forms have done before it, when the people shall become so corrupted as to need despotic government, being incapable of any other."

The everlasting truth that the Constitution would last as long,—and no longer,—as there was any spirit of constitutional morality in the hearts of the people was even better expressed by the great founder of Pennsylvania, when he said:

> "Governments, like clocks, go from the motion men give them and, as governments are made and moved by men, so by men they are ruined, too. Therefore, governments rather depend upon men than men upon governments."

Penn's homely, but forceful, analogy brings us to the very heart of the problem. No constitutional form of government can possibly be maintained unless the people have, not only an intelligent grasp of constitutional principles, but an ever-vigilant and militant purpose to defend them. The purpose of a constitution is not only to create the mechanics of government, but, far more, to subject the passing impulses of a living generation to the reasonable restraints of the collective wisdom of the past. This is impossible unless the individual has some knowledge of the wisdon of the past and a real sense of obligation to the future. Edmund Burke once said that society was a "noble compact between the dead, the living and the unborn." If the living generation lives in the day, there can be no such thing as constitutional morality and, without such morality, no form of government, which attempts to restrain the passing emotions of the day, can possibly survive.

This seems to mark the fatal difference between the present generation and the generation that created the Constitution. I again repeat that the latter thought in terms of abstract political rights, while the living generation thinks only in terms of concrete economics. In other words, the individual today is a pragmatist in the sense that he not only restricts his consideration of any problem to its ponderables, but is often ignorant of the great imponderables that underlie almost any problem.

This is true, not only of the man in the street, but of the more experienced and better educated citizens. Take, for example, the Flexible Tariff proposal, to which I have referred.

Chamber of Commerce after Chamber of Commerce enthusiastically endorsed it because they believed that the President could more speedily and wisely impose tariffs than the Congress. I imagine that this is so, but it is equally true that, if we were to give our Presidents the dictatorial power of a Mussolini and make them, in turn, a Committee of One for the Union, while we might have a more efficient government, free institutions would speedily end, for nothing short of a violent revolution can ever unseat a dictator.

Take a more striking illustration. In the long agitation which culminated in the Eighteenth Amendment, there was interminable discussion as to the ponderables of the problem, but how little consideration was ever given, even by thoughtful Americans, to the undoubted fact that the amendment destroyed the basic principle of our dual form of government by inextricably interweaving the governmental powers of the Federal Government with the reserved police powers of the states. To the pragmatist, the controlling question as to any governmental proposal is, "Does it work an immediate advantage?" Its ultimate effect upon our institutions receives scant or no attention.

The reason for this is very obvious. Life has grown so infinitely complex that it is far more true today than it ever was in Franklin's day that men belong to the "ephemera," of which the sage old Doctor once spoke. We live in the day, forgetful of yesterday and altogether indifferent to the morrow. If any proposal is made that seems to offer a present advantage, the people enthusiastically support it, without considering its possible conflict with all the collective wisdom of the past and its inevitable effect upon the future.

Had the founders of the Republic reasoned in this way, they would have argued that the tax on tea, and the later stamp tax, should be gladly accepted, in return for the great benefit which the Colonies received from the Mother Empire, which protected them in their infancy by her Army and Navy, but the founders of the Republic believed that, if they could be taxed without the consent of their colonial legislatures,

their condition was one of vassalage, for they realized full well, as their English forbears had before them, that the power to tax is the power to destroy. The philosophic mind of Burke realized this unusual capacity of the American people to weigh the imponderables of any problem against the ponderables and the War of Independence, in which our forbears fought for seven weary years for an abstract principle, vindicated his judgment of the American people of that great era.

Of that spirit of constitutional morality there is little evidence today and it is this that has made me so pessimistic as to the perpetuity of our form of government. Each generation of Americans, to gain some immediate and practical advantage, will sacrifice some remaining principle of the Constitution, until that noble edifice will one day become as the Parthenon, beautiful in its ruins, but nevertheless a useless and deserted temple of Liberty.

NEW VERTEBRATE FOSSILS FROM THE LOWER EOCENE OF THE BIGHORN BASIN, WYOMING

By GLENN L. JEPSEN

(*Read April 24, 1930*)

INTRODUCTION

MANY parties have explored the Bighorn Basin of Wyoming for fossil vertebrates during the last twenty years, and yet each season of collecting in this region of vast badlands sees new elements added to the magnificent Lower Eocene Fauna of North America.

As the more accessible areas are temporarily exhausted by the usual "bone hunting" methods, the prospector either turns to fresh territory or examines the old localities with a care which he had before considered extravagant.

That the latter method increases the knowledge of the microfauna particularly, was demonstrated by a fortunate experience of the Scott Fund Expedition of 1927 following the discovery by Mr. T. J. C. von Storch of skeletal parts of a new giant flightless bird at a well-worked locality in the Gray Bull beds about one and one-half miles southeast of Dorsey Creek and perhaps two miles south of the old Otto-Basin road, in Bighorn County. Professor W. J. Sinclair subsequently named this specimen *Omorhamphus storchii*, dedicating the species to its discoverer.[1]

In excavating for the bird bones, members of the expedition found many intermingled pieces of an *Eohippus* skeleton. This discovery led to a meticulous examination of the surrounding area and the observation that the soft incoherent shale matrix swarmed with minute jaws and delicate bones which, when seen from a few feet, closely approximated shale bits in color and shape. Instead of trying the hopeless task of

[1] Sinclair, W. J., 1928, *Proc. Am. Phil. Soc.*, Vol. LXVII, pp. 51–65, Pls. I, II.

working through the loose shale in the field without microscopes, we removed several "blocks" of the crumbly stuff in plaster jackets and shipped them to the laboratory for careful dissection.

The results were not disappointing, for the yield includes four specimens of a new *Diacodon*, *D. tauri-cinerei*, of which the holotype and paratype are herein described, the holotype and paratype of a multituberculate, *Parectypodus tardus*, likewise characterized in the pages which follow, two specimens of *Plagiomene multicuspis* Matthew, part of a minute jaw of *Peradectes* or *Peratherium*, a jaw fragment of a young *Eohippus*, and the skull, jaw, and much of the skeleton of a small *Sinopa*, perhaps *S. viverrina* Cope. Several other broken jaws and many tiny bones that are not determinable also came from the rich blocks whose total surface area in the field had not been more than half a square yard!

Mere chance also governed the discovery of the holotype and paratype of *Parectypodus simpsoni*, to be discussed presently. These remains of what is now the last known straggler of the multituberculates were encountered while members of the 1928 Scott Fund Expedition excavated several intermixed partial skeletons of *Phenacodus primævus*.

Mr. Joseph F. Page '30, a member of the expeditions of 1928 and 1929, had some reward for his unwavering vigilance by discovering a jaw of a diminutive plesiadapid, *Teilhardella chardini*. Diligent future collecting will undoubtedly enlarge the yet incomplete lists of the microfauna of the Wasatch Lower Eocene.

But the new forms are not all small, for to the fauna of the lowest Gray Bull horizon of the Lower Eocene (or so-called Sand Coulee) there must now be added a new species of *Dipsalidictis* (Pl. IV, Figs. 1, 2) larger than *D. platypus* of the Clark Fork Paleocene, and a new *Probathyopsis* which has been named *P. successor*, as it is such both structurally and in time to the Paleocene *Probathyopsis præcursor* Simpson.[1]

The presence of these Paleocene genera in the lower

[1] Simpson, G. G., 1929, "A New Paleocene Uintathere and Molar Evolution in the Amblypoda," *Amer. Mus. Novitates*, No. 387.

Eocene Gray Bull beds may be noted as corroborating the testimony of the stratigraphy that in places the Paleocene-Eocene division is transitional both faunally and physically. Indeed, further evidence may make the distinction between the two levels locally imperceptible except for the appearance of a new group or two of mammals in the Eocene.

"Sand Coulee Beds" was applied as a phase name to the lowest level of the Eocene in the Big and Little Sand Coulee areas by Mr. Walter Granger upon a negative basis [1]—the absence of representatives of the genus *Homogalax* (*Systemodon*) which appears abundantly in the overlying Gray Bull beds. As typical *Homogalax* was found by the Princeton Scott Fund Expedition of 1928 in "Sand Coulee" beds, the distinction between them and the Gray Bull proper is no longer valid. But Mr. Granger was fully justified in his conclusion in 1914, for *Homogalax* is exceedingly rare in the lowest Gray Bull. To date, only one good specimen has been found. This newly-known range of *Homogalax* simply implies that the great abundance of the genus just above the lowest Gray Bull beds is the result of a sudden expansion and not a migratory invasion. No doubt this is true of other forms which suddenly 'burst' into the attention of the paleontologist.

The object of the present paper is to record the undescribed material secured from the Gray Bull Lower Eocene of Park and Bighorn Counties, Wyoming, by the Princeton Scott Fund Expeditions of 1927, 1928, and 1929. The work has been most generously supervised by Professor W. J. Sinclair. The drawings are by Mr. Loris S. Russell.

[1] Granger, W., 1914, "On the Names of Lower Eocene Faunal Horizons of Wyoming and New Mexico," *Bull. Amer. Mus. Nat. Hist.*, Vol. XXXIII, Art. XV, p. 205.

NEW FORMS DESCRIBED

MULTITUBERCULATA

Ptilodontidæ

Parectypodus simpsoni, new genus and species.[1]

Plate I, Figs. 1–3; Plate II, Fig. 8

HOLOTYPE.—Princeton No. 13242, a left lower jaw.

PARATYPE.—Princeton 13242a, a left P_4 imbedded in a jaw fragment from the same level and locality as the type.

HORIZON AND LOCALITY.—Gray Bull beds, Lower Eocene, between Elk Creek and "South Elk Creek," Bighorn County, Wyoming.

DIAGNOSTIC CHARACTERS.—Lower dental formula 1.0.1.2. No depression or groove on the anterior end of P_4, which is relatively narrower than that of *Ectypodus*. Eight external and four internal cusps on M_1. Otherwise the general appearance of the teeth and jaw is similar to *Ptilodus* and *Ectypodus*, both of which, however, have P_3.

The general shape and sculpturing into ridges and furrows of P_4 in *Ectypodus* is very like that of *Parectypodus*. The former probably has a minute P_3, much smaller than that of *Ptilodus*, but similarly accommodated in a depression in the anterior end of P_4. In the three specimens of *Parectypodus* having P_4, there is no trace of this distinctive modification, the anterior base of P_4 being smoothly rounded, with no concavity. The cutting edge of P_4 is serrated by the superior extensions of the 14 ridges modifying its surface on each side.

M_1 of *Parectypodus* is shorter, wider, and more robust than that of *Ectypodus*. The eight external cusps, as far as discernible through their advanced wear, are like those of *Ectypodus* but the inner cusps differ in number and size, there being four large cusps in *Parectypodus*, increasing in size posteriorly, whereas *Ectypodus* possesses six much smaller inner cusps on M_1.

Although the absolute size of the incisor of *Parectypodus* is greater than that of *Ectypodus*, relative to the jaw, the latter has an incisor which is not only larger, but has its roots encased in a heavier and shorter collar of bone than has *Parectypodus*. In cross section, the ramus of *Ectypodus*, anterior to the premolars, is compressed, ovoid, far greater in depth than in width, whereas a similar section of the jaw of *Parectypodus* reveals a more rounded

[1] *Parectypodus* = near *Ectypodus*. The species is named in honor of Dr. G. G. Simpson.

outline. Also the symphysial scar of the jaw of *Parectypodus* is larger than that of *Ectypodus*.

This species is the last survivor yet discovered of the multituberculates, since *Parectypodus tardus* comes from a somewhat lower level in the Gray Bull, and *Neoliotomus* occurs close to the base of the beds. Future discoveries, however, may give the group a range much further up into the Tertiary.

Parectypodus tardus, new species.

Plate II, Figs. 1–4

HOLOTYPE.—Princeton No. 13265, a right lower jaw containing P_4–M_2.

PARATYPE.—Princeton No. 13265a, a left M_1, from the same locality, a few inches from the holotype.

HORIZON AND LOCALITY.—Gray Bull beds, Lower Eocene, about one and one-half miles southeast of Dorsey Creek and two miles south of the old Otto-Basin road, Bighorn County, Wyoming. From a "pocket" in soft shale rich in fossil bones, at a lower level in the Gray Bull than that from which *P. simpsoni* was collected.

DIAGNOSTIC CHARACTERS.—Dental formula like that of *P. simpsoni*, 1.0.1.2, with 10 serrations, or saw-tooth ridges, on the cutting edge of P_4. Four external cusps (the posterior two illy defined) and two internal cusps on M_2. Lower outline of jaw plunges downward toward the front from a point below the middle of M_1, making the jaw depth in front of P_4 relatively much greater than it is in *P. simpsoni*.

On P_4 the serrations and the spaces between the ridges increase in size from the front toward the back much more markedly than in *P. simpsoni*. The widths of P_4 and M_1 are relatively greater in *P. simpsoni*. The cuspidation of M_1 is similar. *P. tardus* is about 75 per cent as large as *P. simpsoni*.

The cutting edge of P_4 makes nearly a straight line with the lingual cusp row of M_1 in *P. tardus*. In *P. simpsoni*, if a line along the crest of P_4 be continued posteriorly, it passes on the lingual side of M_1, whose axis, if projected posteriorly, intercepts this line slightly back of M_1. In other words, the axes of P_4 and M_1 in *P. tardus* are parallel, almost coincident, whereas in *P. simpsoni*, P_4 and M_1 are set at angles in the jaw so that the axes of the two teeth are at an angle (cf. Pl. I, Fig. 1 and Pl. II, Fig. 4). Paratype No. 13265a consists of a worn left M_1, evidently not of the same individual as the holotype which has unworn teeth, though the two were

found but a few inches apart. The posterior outline of M_1 is rounded, fitting close into the concave anterior margin of M_2. These two teeth, M_1 and M_2, present a gradual increase in width posteriorly, as can be seen from the figures.

MEASUREMENTS OF *Parectypodus*

	P. simpsoni		*P. tardus*	
	13242 mm.	13242a mm.	13265 mm.	13265a mm.
Tip of I—posterior alveolus of M_2	14.8			
Length of P-M series	7.6		5.6	
P_4 length	4.5	4.0	3.0	
width	1.5	1.7	0.8	
height	3.6	3.6	2.1	
M_1 length	2.3		1.9	1.8
width	1.1		0.8	0.8
height (enamel on lingual side)	0.7		0.4	0.4
M_2 length			1.1	
width			1.0	
height			0.5	
Depth of jaw at anterior edge of P_4	3.5		2.7	
Depth of jaw below M_1	4.8		2.9	
Length of I (tip to alveolus, labial side)	5.4			
Diastema I–P_4, labial side	3.6			

Neoliotomus conventus, new genus and species.

Plate IV, Figs. 3–7

HOLOTYPE.—Princeton No. 13297. Left lower jaw fragment with broken I and P_4, and root of P_3; broken upper left M^1; part of right maxilla with root of P^3 and broken P^4, and showing the base of the malar process.

HORIZON AND LOCALITY.—Lower Wasatch (Gray Bull beds), west of Clark's Fork of the Yellowstone, on the divide between Paint and Pat O'Hara Creeks, just above contact with Clark Fork beds, Park County, Wyoming.

DIAGNOSTIC CHARACTERS.—I compressed laterally, with a limited band of enamel rounding the anterior edge. A small P_3 present, accommodated in an anterobasal depression of P_4. Eleven serrations show upon each side of the preserved portion of P_4. The approximate length of this tooth should be 14 mm. if the base of the tenth serration is on about the middle of the tooth, as it is on most ptilodontids having 14 or 15 serrations on P_4. Enamel covers more

of the root on the labial than on the lingual side. Several excessively minute cuspules modify the most anterior ridge of each side. In general the tooth is about 25 per cent larger than P_4 of *Eucosmodon* (= *Neoliotmus*) *ultimus* (Granger and Simpson).[1]

A bit of a root of P^3 is close to the anterior root of P^4, the latter having one anterior and two posterior roots. P^4 is somewhat like the described P^4 of *Liotomus marshi* (Lemoine, 1883), and agrees with it in being similarly different from all other multituberculate sectorials. Judging from the roots, about the posterior third of the crown is missing, but the remaining part is triangular in cross section, and has a single cutting edge supporting eight cusps which continue upon each side as short ridges. The anterior part of the tooth is rugose, and just labial to the third cusp of the main row is a large low accessory cusp which widens this part of the tooth. A large anterior basal notch shows wear upon the enameled part of it, probably from a slight movement against the somewhat underlying P^3.

It is impossible to tell how much of the crown of M^1 is missing but the preserved part presents three rows of cusps, the labial row having 7, the middle and lingual each showing 8. A ledge at the base of the three anterior cusps of the labial row becomes more prominent anteriorly and rises to a ridge at the anterolabial angle of the tooth. The inner row of cusps approaches the other two rows in number and size, being more complete than in other described Tertiary ptilodontids.

The malar process of the maxilla arises external to the base of P^4, much as it does in *Eucosmodon*[2] though in the latter it is a bit larger and more anteriorly placed.

MEASUREMENTS

	mm.		mm.
I depth	6.4	P_4 width	4.2
width	2.5	length (estimated)	14.0
Diastema I–P_3	6.0	height of enamel	8.0
P_3 root width	2.9		

It is important to note that all of the parts of this specimen were found within an area of a few square feet, and probably belong to one individual. A comparison with the specimen in the American Museum described in 1928 as *Eucosmodon ultimus* reveals that the

[1] Granger, W. and Simpson, G. G., 1928, "Multituberculates in the Wasatch Formation," *Amer. Mus. Novitates*, No. 312.

[2] Observed in American Museum No. 16534, *Eucosmodon* sp.

two differ only specifically, the dental formula being the same, as far as preserved, although when described, the alveolus of P_3 of the American Museum specimen had not been observed. There are slight differences in the shape of the jaw between the incisor and the third premolar, possibly magnified by crushing in either or both specimens, and a difference of about 25 per cent in size, the individual herein described being the larger. Both are from about the same level of the lower Wasatch. Also occuring in this formation are other large multituberculates, represented at present solely by broken incisors. These incisors are smaller than those of *Neoliotomus* and wear in a different manner, to a sharper point. It seems unlikely that they are upper incisors of *Neoliotomus*.

The similarity of the P^4 to that of *Liotomus* has already been indicated. The differences, as far as determinable upon single teeth, are specific, but the great geographic separation and the fact that *Neoliotomus* is from the Eocene, whereas *Liotomus* comes from the Paleocene (Thanetian) seem to warrant a tentative generic distinction, pending the discovery of additional material of both forms.

INSECTIVORA

Leptictidae

Diacodon tauri-cinerei, new species.[1]

Plate III, Figs. 1–4

HOLOTYPE.—Princeton Collection No. 13104, well-preserved jaws and crushed skull.

PARATYPE.—Princeton Collection No. 13267, lower jaws with incisors preserved on the right side. Same locality and horizon as the type.

HORIZON AND LOCALITY.—Lower Eocene Gray Bull beds, south of Dorsey Creek, Bighorn County, Wyoming, from the same locality and shale "pocket" as *Parectypodus tardus*.

DIAGNOSTIC CHARACTERS.—Dental formula $\frac{3\ 1\ 4\ 3}{3\ 1\ 4\ 3}$. Lower incisors and canine are compressed and set enechelon, touching. Incisors increase in size posteriorly. Canine has a chisel-like tip, not pointed, leans forward and is shaped much like the single-rooted P_1, although the latter is shorter and has a more prominent small posterior basal cusp which fits into the anterior basal concavity of P_2 when the teeth are completely set in their alveoli. P_2 has two roots,

[1] The *Diacodon* of the Gray Bull beds.

and in the holotype, but not in other specimens, a tiny anterior cusp. On all specimens this tooth has a large basal posterior cusp and directly behind it a very small cusp. On P_3 there is a sharp anterior basal cusp which is not present in *Diacodon bicuspis* (Cope) or *Diacodon alticuspis* Cope, but may be seen in *D.* (*Prodiacodon*) *puercensis* Matthew and Granger,[1] and is observed in a small undescribed Wyoming Paleocene specimen in the Princeton Collection. The main cusp supports upon its posterior edge a cusp which is larger and higher than the corresponding structure in *D. bicuspis*, and, at the extreme posterior edge of the tooth, there is a small fourth cusp in line with the other three. The anterior cusp of P_4 is wider and less conical than it is in *D. bicuspis*, and the hypoconulid is more prominent. The molar trigonids are high, and the metaconids are higher than the other trigonid cusps. Hypoconulids are distinct and increasingly large posteriorly. In other respects the lower molars are very similar to those of previously described Diacodons.

Upper teeth.—Two incisors are preserved. They are compressed and gently rounded, not pointed. The more anterior one is the larger and higher. The canine is comparatively long and slender for a *Diacodon*. It is separated from the incisors and from P^1 by short diastemata. P^1 had two roots, as shown by the alveoli. P^2 is double-rooted and has a very small anterior cusp and a posterior basal cusp. Three rooted, P^3 has a rounded triangular outline, each side being concave in its middle part. Of the well developed paracone, metacone, and protocone, the latter is the largest. The anterior labial cusp (the parastyle) is well developed and by itself constitutes an anterior spur on the tooth. A posterolabial cingulum enlarges to a small metastyle. Small cingula are at the anterior and posterior base of the protocone, but not on the lingual portion, and the whole cusp is much larger than that on the corresponding tooth of *D. bicuspis*. In outline, P^4 and the first molars have strikingly concave posterior borders, due to the metastyles and the spurlike development of the hypocones. On the molars the protocones are compressed anteroposteriorly, and lean forward. Compared with other Diacodons, this species has molars with unusually small anteroposterior diameters and great linguolabial dimensions. All of the parastyles are greatly developed, as are the metastyles and the external cingula.

[1] Matthew, W. D., and Granger, W., 1918, "A Revision of the Lower Eocene Wasatch and Wind River Faunas," *Bull. Amer. Mus. Nat. Hist.*, Vol. XXXVII, Art. XVI, pp. 571–9. Matthew, W. D., "Preoccupied Names," *Journal of Mammalogy*, Vol. 10, No. 2, p. 171, May, 1919.

MEASUREMENTS

	mm.
P_1–M_3	17.6
M_1–M_3	7.3
Depth of jaw under M_3	4.5
P^1–M^3	17.0
M^1–M^3	6.4

PRIMATES

Tarsiidæ

Tetonius tenuiculus, new species.

Plate II, Fig. 9

HOLOTYPE.—Princeton No. 13027. Skull fragment with right P^4–M^3.

HORIZON AND LOCALITY.—Lower Gray Bull beds, Bighorn Basin, Wyoming, about two miles east of the old Otto-Basin road-crossing on Dorsey Creek and well south of the road.

DIAGNOSTIC CHARACTERS.—P^4 with high paracone, more anteriorly placed than in *Tetonius homunculus*,[1] no parastyle, very small metastyle, rudimentary hypocone, basal cingulum complete except for small interruptions on lingual side of protocone and at the posterolabial corner of the tooth. The transverse dimensions of the molars are less in proportion to their anteroposterior diameters than in *T. homunculus*. The ridges from the paraconules and the metaconules to the paracones and the metacones, respectively, are small but distinct on all the molars. In other respects, only its smaller size distinguishes this species from *Tetonius homunculus*.

MEASUREMENTS

	mm.
P^4–M^3	5.7
M^1–M^3	4.4
M^2 width	2.9

PRIMATES?

Plesiadapidæ

Teilhardella chardini, new genus and species.[2]

Plate II, Figs. 5–7

HOLOTYPE.—Princeton No. 13236, right lower jaw with P_4 and M_3, and roots of I, P_3, M_1, and M_2.

[1] Matthew, W. D., and Granger, W., 1915, "A Revision of the Lower Eocene Wasatch and Wind River Faunas," *Bull. Amer. Mus. of Nat. Hist.*, Vol. XXXIV, Art. XIV, 457–63.

[2] Named in honor of Prof. Pierre Teilhard de Chardin, President of the Geological Society of France and Professor of Geology in the Catholic Institute of Paris.

HORIZON AND LOCALITY.—Gray Bull beds, Lower Eocene, Elk Creek, Bighorn Basin, Wyoming. Found by Joseph Page, August 30, 1928.

DIAGNOSTIC CHARACTERS.—Dental formula, $\frac{?\ ?\ ?\ ?}{1.0.2.3}$. The incisor much enlarged, its root extending posteriorly and ending in a point below the middle of the last molar. Jaw very short and deep. A depression under P_4 as in *Heterohyus nanus* Teilhard de Chardin.[1] Posterior mental foramen below M_2. P_3 single rooted, and on a level below that of the remaining P–M series. Although the crown of P_3 is broken off in the specimen, the proclivous attitude of the root (being almost parallel with the incisor root), indicates that, from its base, the tooth slanted forward and upward, and then downward. The crown at its anterior margin may have rested upon the incisor, in a position analogous to that of P_4 in *Stehlinella uintensis* (Matthew).[2] A short diastema between P_3 and P_4. P_4 double-rooted, in close contact with the first molar and of the same height as the molar row. This tooth has a large protoconid, a small paraconid, and a metaconid so minute that it is a mere bulge on the inner side of the base of the protoconid. The heel is deeply basined and has a cuspidate margin. Though the crown of M_1 is missing, the roots remain and the posterior one is much larger than the anterior. The specimen lacks M_2 entirely. M_3 with large protoconid and metaconid. Paraconid smaller and more internally placed than the metaconid. The trigonid has the rectangular shape characteristic of most Plesiadapids, due to the anterior flare at the base of the protoconid. The heel is lower, longer, and narrower than the trigonid. It is basin-shaped and has no trace of the posterior spur which is developed in *Heterohyus*.

MEASUREMENTS

	mm.
P_4-M_3	5.7
M_3 (length)	1.7
M_3 (width)	1.2

[1] "Les Mammiféres de l'Eocene Inférieur Francais et leurs Gisements," *Annales de Paléontologie*, t. X, 93, 1916–1921.

[2] W. D. Matthew, "*Stehlinius*, A New Eocene Insectivore," *Amer. Mus. Novitates*, No. 14, September 7, 1921: W. D. Matthew, "Preoccupied Names," *Journal of Mammalogy*, Vol. 10, No. 2, p. 171, May, 1929.

CREODONTA

Oxyænidæ

Dipsalidictis amplus, new species.

Plate IV, Figs. 1, 2

HOLOTYPE.—Princeton No. 13153, parts of both maxillæ and lower jaws.
HORIZON AND LOCALITY.—Lower Wasatch Gray Bull beds, Big Sand Coulee Basin, Park County, Wyoming, in T. 56 N., R. 101 W., Sec. 31.
DIAGNOSTIC CHARACTERS.—The generic characters of the specimen agree absolutely with those designated by Matthew for the genus:—"Deuterocone on P^4 only; M^2 transverse, unreduced, M^3 absent; M_1 and M_3 subequal, tuberculosectorial with large basin heels, M_3 absent; P_1 one-rooted; anteroexternal cusp of P_4 prominent . . . ."

The species is larger than *Dipsalidictis platypus* and has somewhat different tooth proportions. Although M^2 has nearly the same anteroposterior diameter in both species, the transverse diameter is much greater in this new form. The cuspidation of the two species is very similar, but in *D. amplus* there is a posterior lobe upon the posterolabial cusp of P_4 which is lacking in *D. platypus*.

MEASUREMENTS

	mm.
M^1	17.1
P^3–M^2	37.0
P^4–(diameters, ap. × tr.)	12.1 × 11.5
M^1–(diameters, ap. × tr.)	10 × 13.6
M^2–(diameters, ap. × tr.)	4.8 × 14
M_{1-2}	20.5
P_4 length	10.4
M_1 length	9.8
M_2 length	12.3

AMBLYPODA

Uintatheriidæ

Probathyopsis successor, new species.

Plate IV, Figs. 8–11

HOLOTYPE.—Princeton No. 13234, skull and jaw fragments with most of the upper dentition and several lower teeth preserved.
HORIZON AND LOCALITY.—Lower Gray Bull beds, Lower Eocene, T55N, R101W, Sec. 2, Park County, Wyoming.
DIAGNOSTIC CHARACTERS.—This species has many characters in common with both *Probathyopsis praecursor*[1] and *Prodinoceras*

[1] Simpson, G. G., 1929, "A New Paleocene Uintathere and Molar Evolution in the Amblypoda," *Amer. Mus. Novitates*, No. 387.

martyr,[1] and differs only slightly from each. The enameled portion of the upper canine is straight and much like that of *Prodinoceras*, and the root is curved and extremely long (Plate 4, Fig. 9). P^2 may or may not have been set in the maxilla at an angle (". with ectoloph turned inward anteriorly at 45 degrees to tooth row . . ." [2]) as it is in *Prodinoceras*. Certainly, in outline, these two teeth resemble each other more than either one approximates P^2 of *Probathyopsis praecursor*. But in both species of *Probathyopsis* P^2 is unbasined and has a single crest from the inner cusp to the ectoloph, whereas in *Prodinoceras* this is true for the left side only, the right being as described by Simpson for the species, ". . . metacone indicated but not rising free of ectoloph, protocone distinct, lower than metacone, united to paracone by a strong crest and to metacone by a weaker one, enclosing a small basin."

In the drawing of the left upper cheek teeth (Plate 4, Fig. 10) P^3 is drawn reversed from the right side. P^2 is also from the right side, but by mistake, is not transposed.

Unlike *Prodinoceras*, the cingulum is complete on M^2 and M^3, and these teeth have no parastyles. From the middle of the metaloph on each molar a prominent ridge descends to a depression in the posterior cingulum. A small similar ridge is present upon the third molar of *Probathyopsis praecursor*, but is totally absent from M^1 and M^2.

If the order of the lower incisors has been correctly deduced (Plate IV, Fig. 11), the first is small and rounded, the second is higher and more compressed, and the third is even larger. Each of the latter two is blunt pointed, has a rounded medial ridge upon the lingual side from base to tip and seems to have a complete basal cingulum. The lower canine is very small and simple, with sharp, curving anterior and posterior edges (Plate IV, Fig. 8).

The posterior part of the heel of M_3 is wider, more rounded, and smoother than it is in *P. praecursor*.

Probathyopsis is another example of a genus which bridges the Paleocene-Eocene division. An undescribed species in the Princeton Collection from the Clark Fork is much like *P. praecursor*, and less similar to the species herein described, the lower teeth of the three specimens making, as far as can be ascertained, a superb example of an evolutionary sequence, (1) *P.* sp. (Clark Fork), (2) *P. praecursor* (Clark Fork), (3) *P. successor* (Gray Bull).

[1] Simpson, G. G., 1929, "Additions to the Fauna of the Gashato Formation of Mongolia," *Amer. Mus. Novitates*, No. 376, pp. 10-11.

[2] Ibid., p. 10.

EXPLANATION OF PLATES

For convenience in comparing measurements, each line near a drawing is the actual length between two points, indicated by arrows, on the specimen.

PLATE I

FIG. 1.—*Parectypodus simpsoni.* Holotype, new genus and species. Princeton No. 13242. Left lower jaw, crown view, × 4.2.

FIG. 2.—Internal view of the same, × 4.4.

FIG. 3.—External view of the same, × 4.1.

PLATE II

FIG. 1.—*Parectypodus tardus.* Holotype, new species, Princeton No. 13265. Right lower jaw, internal view, × 6.5.

FIG. 2.—External view of the same, × 6.5.

FIG. 3.—*Parectypodus tardus.* Paratype. Princeton No. 13265a. Left lower first molar, crown view, × 6.5.

FIG. 4.—*Parectypodus tardus.* Holotype. Right lower jaw, crown view, × 6.5.

FIGS. 5, 6, 7.—*Teilhardella chardini.* Holotype, new genus and species. Princeton No. 13236. Right lower jaw, external, crown, and internal views, × 6.2.

FIG. 8.—*Parectypodus simpsoni.* Paratype. Princeton No. 13242a. Left lower fourth premolar in jaw fragment, internal view, × 6.

FIG. 9.—*Tetonius tenuiculus.* Holotype, new species. Princeton No. 13027. Right upper molars and fourth premolar, × 4.

PLATE III

FIG. 1.—*Diacodon tauri-cinerei.* Holotype, new species. Princeton No. 13104. Composite drawing, for which teeth of both sides have been used, of right upper dentition, × 4.

FIG. 2.—*Diacodon tauri-cinerei.* Holotype, new species. Right lower dentition, crown view, × 4.

FIG. 3.—*Diacodon tauri-cinerei.* Paratype. Princeton No. 13267. Right lower incisors, canine, and anterior premolars, external view, × 4. P_3 is reversed from the left side.

FIG. 4.—*Diacodon tauri-cinerei.* Holotype, new species. Princeton No. 13104. External view of right lower jaw, × 4.

PLATE IV

FIG. 1.—*Dipsalidictis amplus.* Holotype, new species. Princeton No. 13153. Left lower jaw with P_3–M_2, outer side. The positions of the double-rooted P_2 and single-rooted P_1 are indicated in outline from a fragment of the opposite side, × 1/1.

FIG. 2.—*Dipsalidictis amplus.* Holotype. Right upper P^3–M^2, crown view, × 1/1.

FIG. 3.—*Neoliotomus conventus.* Holotype. New genus and species. Princeton No. 13297. Left lower jaw, crown view, showing incomplete P_4 and roots of the incisor and P_3, about × 2.

FIG. 4.—*Neoliotomus conventus.* Holotype. Left lower jaw, external view, × 2.1.

FIG. 5.—*Neoliotomus conventus.* Holotype. Incomplete left upper M^1, crown view, × 2.

FIG. 6.—*Neoliotomus conventus*. Holotype. Part of right maxilla with root of P^3 and broken P^4, showing base of malar process, × 2.

FIG. 7.—*Neoliotomus conventus*. Holotype. Incomplete upper P^4, right, crown view, × 2.

FIG. 8.—*Probathyopsis successor*. Holotype. New species. Princeton No. 13234. Left lower canine, external view, × 1/1.

FIG. 9.—*Probathyopsis successor*. Holotype. Right upper canine, external view, × 1/2.

FIG. 10.—*Probathyopsis successor*. Holotype. Upper dentition, left side, crown view, × 1/1. P^3 is reversed from the opposite side. P^2 is also from the opposite side but, by mistake, is not transposed. The point in contact with P^3 is the anterior edge of P^2 right.

FIG. 11.—*Probathyopsis successor*. Holotype. Lower incisors, supposedly of the right side, external view, × 1/1.

DEPARTMENT OF GEOLOGY
PRINCETON UNIVERSITY
PRINCETON, N. J.

The William Berryman Scott Research Fund.

PLATE I

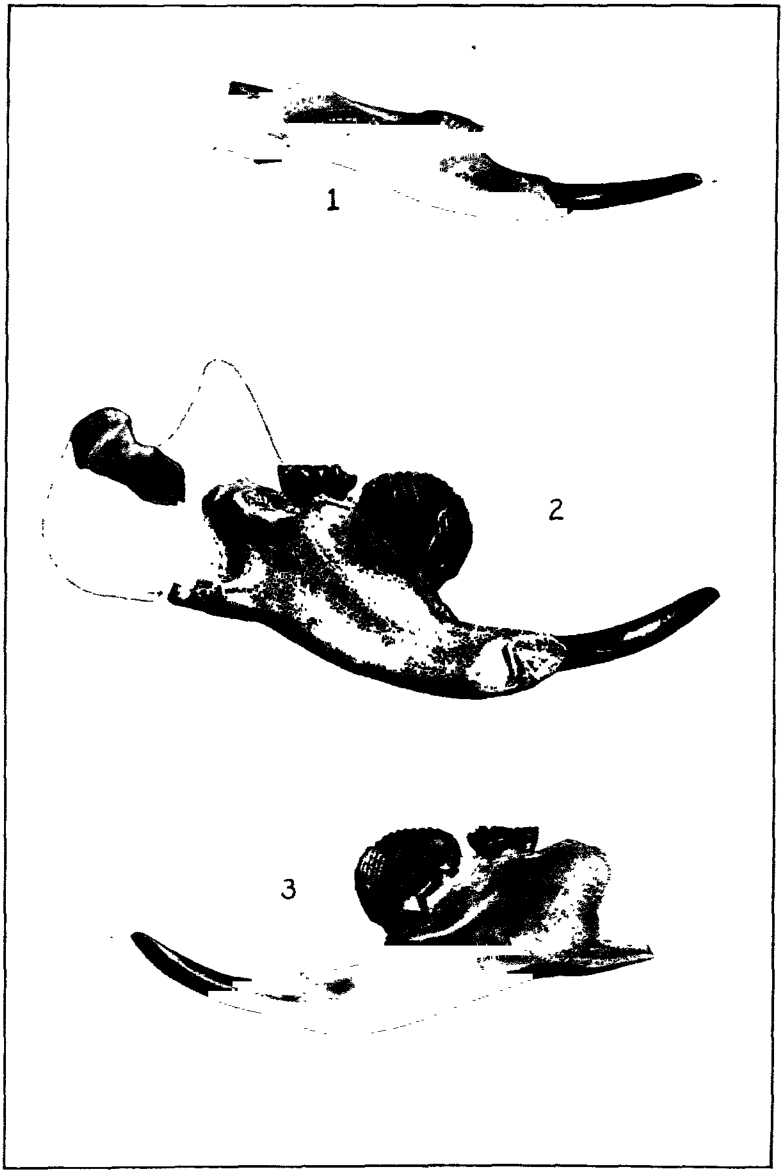

PLATE II

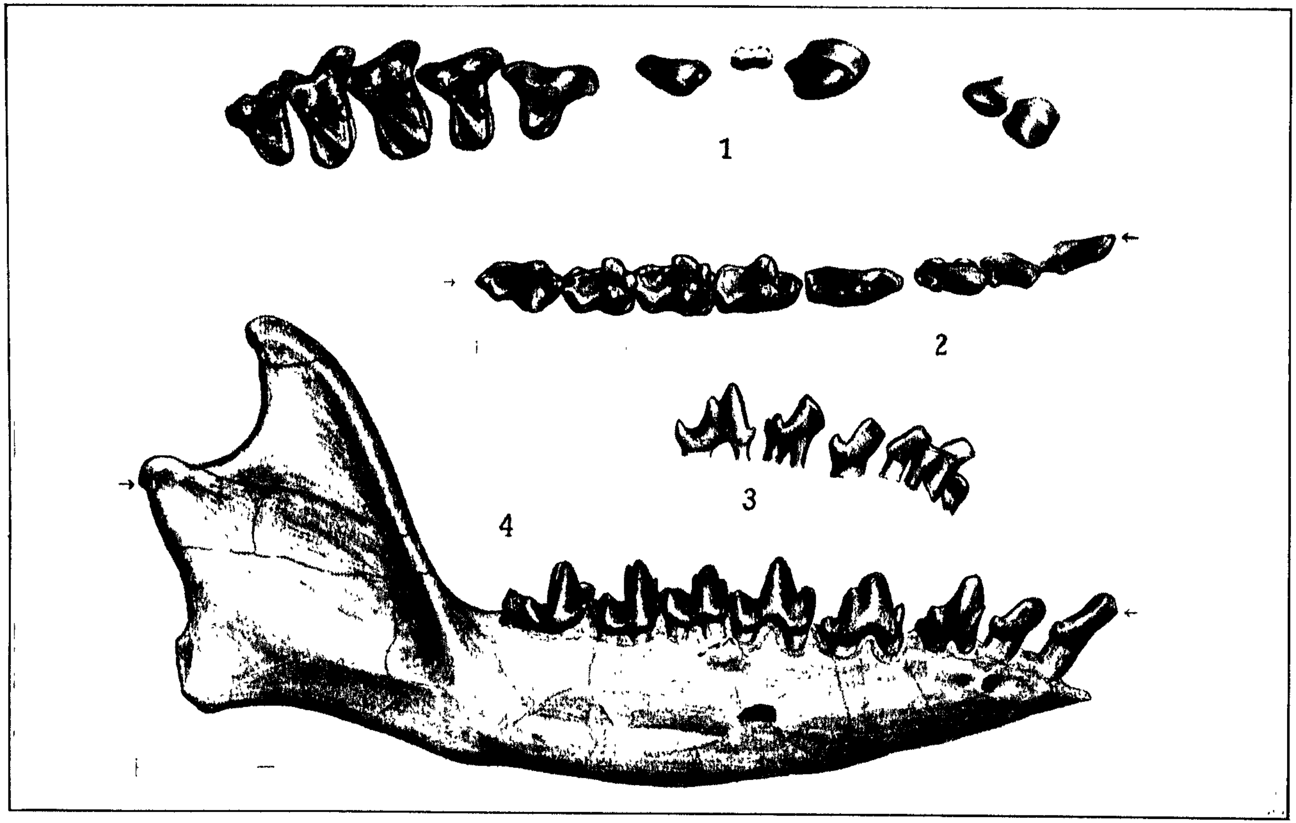
1
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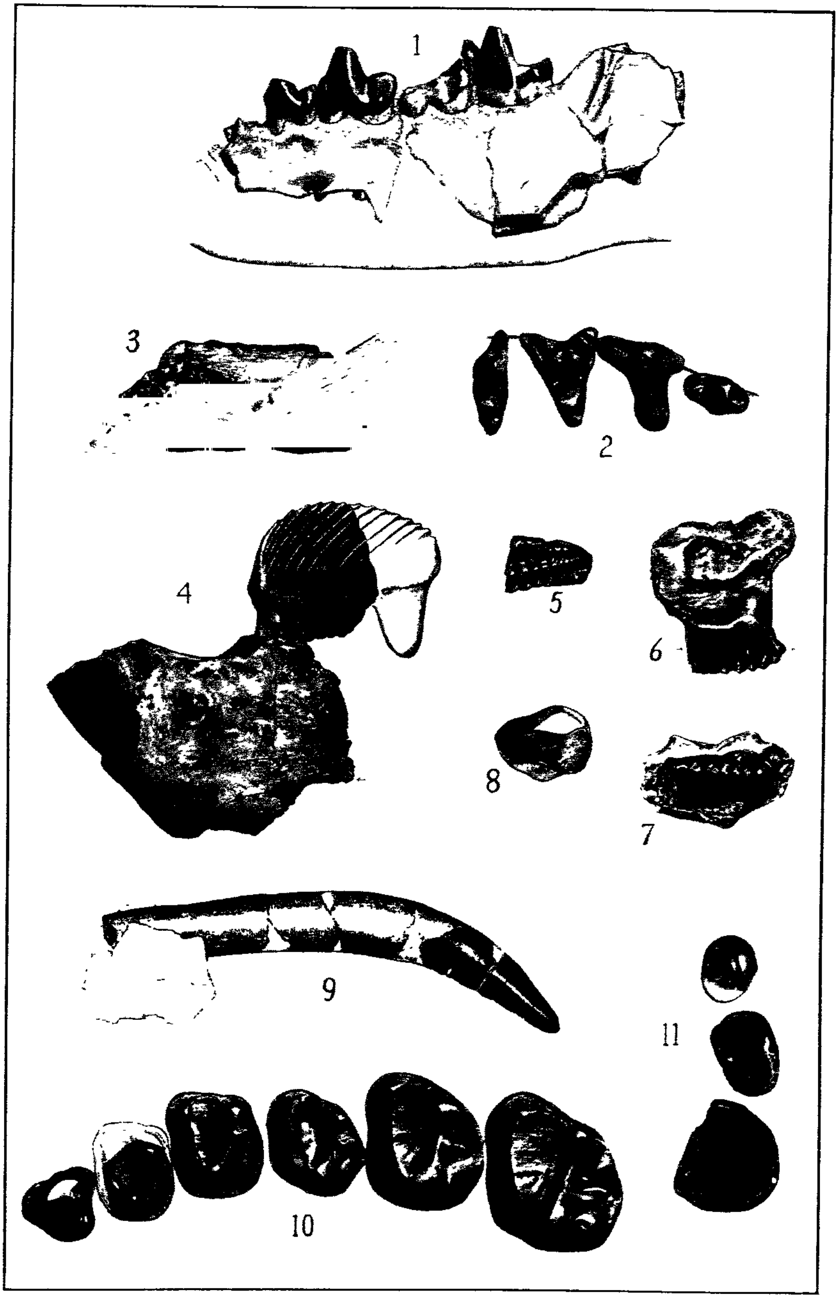
PLATE IV



UPPER CRETACEOUS DINOSAUR FAUNAS OF NORTH AMERICA

By LORIS S. RUSSELL

(*Read February 7, 1930*)

THERE is an unfortunate tendency on the part of some palæontologists to discuss the fossil vertebrates of North America without sufficient regard for their stratigraphic occurrence. Probably the dinosaurs have suffered as much in this respect as any group, notwithstanding the fact that the geological history of these reptiles in certain epochs is rather well known. This is true particularly for the late Cretaceous of North America, on which continent we find a number of Upper Cretaceous sections with successive faunas rich in dinosaurian remains.

The writer has endeavored to compile in the present paper most of the available stratigraphic data on these dinosaurian faunas. Professor W. T. Thom, of Princeton University, and Mr. C. W. Gilmore and Dr. J. B. Reeside, Jr., of the United States National Museum, have given valuable assistance. In discussing the various dinosaur-bearing sections the arrangement is primarily by states and provinces. The order of presentation is somewhat unsystematic, but is designed to aid in the understanding of the correlation. The descriptions are supplemented by a map and a correlation table.

NEW JERSEY

The Upper Cretaceous rocks of New Jersey are among the earliest-known sources of dinosaurian remains in North America. Joseph Leidy described adequate material from this state as early as 1858, and subsequent work on the same field was published by E. D. Cope and O. C. Marsh.

The stratigraphy of the New Jersey Cretaceous has been described in the late Stuart Weller's monograph [1] on the

[1] Geol. Surv. New Jersey, Paleont. Ser., Vol. 4, 1907.

Western Europe	New Jersey	Wyoming	Central Montana	North-western Montana	Southern Alberta	Southern Saskatchewan	Colorado	North-western New Mexico
	Hornerstown (Eocene)	Fort Union (Paleocene)	Tullock (Paleocene?)	Willow Creek (Paleocene)	Paskapoo (Paleocene)	Ravenscrag	?	Puerco (Paleocene)
Danian	break	Lance	Hell Creek	St. Mary River	break	Ravenscrag	Denver Arapahoe Laramie	break
Maestrichtian	Tinton Red Bank	Fox Hills	Hell Creek Lennep	St. Mary River Horsethief	Edmonton	break Whitemud Basal sandstone	"Fox Hills" ?	Ojo Alamo Kirtland Fruitland Pictured Cliffs
Senonian	Navesink Mt. Laurel Wenonah Marshalltown Englishtown Woodbury Merchantville Magothy	Pierre	Bearpaw Judith River Claggett Eagle	Bearpaw Two Medicine Virgelle	Bearpaw Belly River: Pale beds, Foremost, Pakowki, Milk River	Bearpaw		Lewis Mesaverde Mancos
Senonian	break?	Niobrara	Colorado	Colorado	Colorado			
Turonian	break?	Benton	Colorado	Colorado	Colorado			
Cenomanian	? Raritan ?	Dakota						

NOTE
The formations listed above the Danian stage are not correlated with each other.

TABLE I.—Correlation of Upper Cretaceous dinosaur-bearing formations of North America, as given in the present paper.

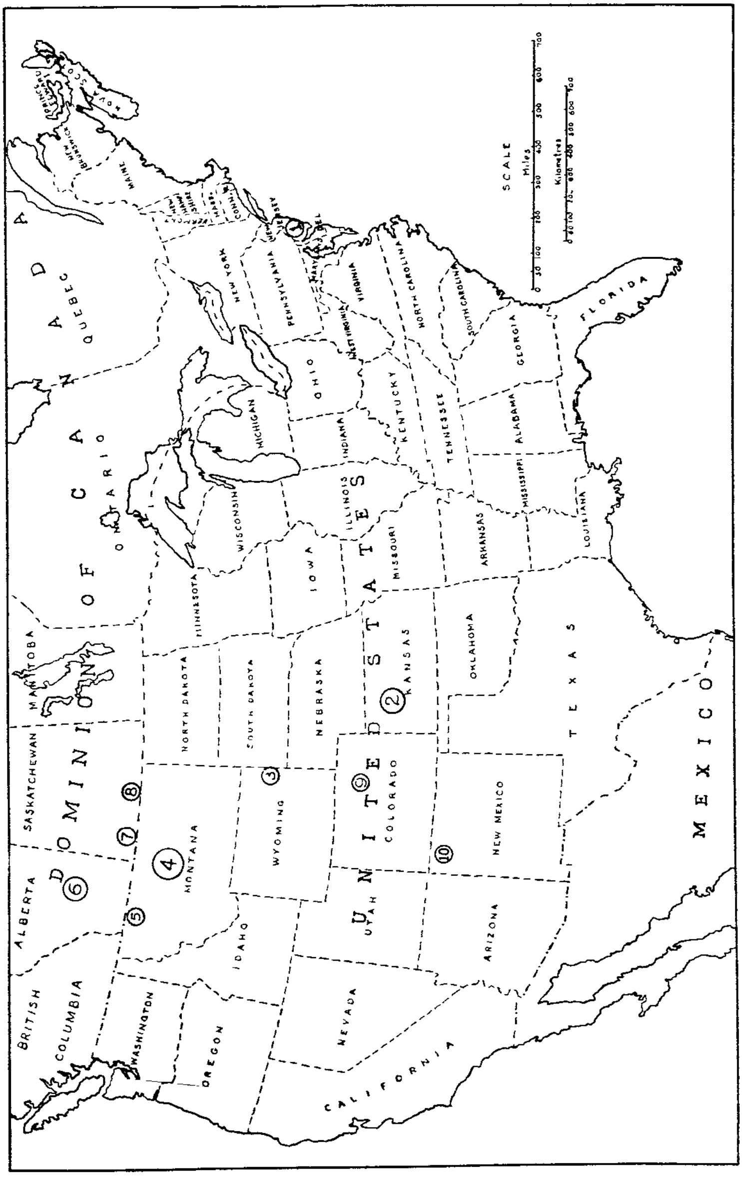


FIG. 1.—Outline map of the United States and southern Canada, with location of Upper Cretaceous dinosaur fields marked by circles and numbers, as follows: 1, coastal plain of New Jersey; 2, Niobrara formation of western Kansas; 3, Lance formation of Niobrara Co., Wyoming; 4, Judith River and Hell Creek formations of central Montana (only part of area is indicated by circle); 5, Two Medicine and St. Mary River formations of Glacier Co., Montana; 6, Pale beds and Edmonton formation on Red Deer river, Alberta; 7, lower Ravenscrag beds of Cypress Hills, Saskatchewan; 8, lower Ravenscrag beds of Wood Mountain, Saskatchewan; 9, Laramie, Arapahoe and Denver formations of the Denver basin, Colorado; 10, Fruitland, Kirtland and Ojo Alamo formations of San Juan Co., New Mexico.

Cretaceous palæontology of this state. The same section has been studied more recently by Messrs. C. W. Cooke and L. W. Stephenson,[1] who have transferred the two uppermost formations of Weller's Cretaceous series to the Eocene. The succession, as now understood, is shown in the second column of the accompanying correlation table.

The lowermost division of this section is the Raritan formation, which has been correlated,[2] on the basis of flora, with the Cenomanian. Above this the rocks are predominantly of marine deposition, with fairly abundant invertebrate fossils. Weller [3] referred these fossils to a single major fauna, which he called the Ripleyian. This fauna is partly Senonian, and partly Mæstrichtian, in age.

In listing the dinosaurian fauna from these Upper Cretaceous formations, considerable difficulty is experienced, due to incomplete locality data, in placing the various species in their true horizons. Since more than one formation is represented, such stratigraphic and locality information as can be obtained is included in the list. Space does not permit the inclusion of references or synonymies, but most of this information may be obtained from Dr. O. P. Hay's bibliography.[4]

LIST OF UPPER CRETACEOUS DINOSAURS FROM NEW JERSEY

THEROPODA

Deinodontidæ

Dryptosaurus aquilunguis (Cope). Navesink? formation, near Barnsboro, Gloucester County.

Ornithomimidæ

Coelosaurus antiquus Leidy. (incl. "*Laelaps*" *macropus* Cope) Monmouth and Burlington Counties.

[1] *Jour. Geol*, Vol. 36, pp. 139–148, 1928.
[2] Berry, E. W., *Jour. Geol.*, Vol. 18, pp. 252–258, 1912.
[3] Op. cit., p. 179.
[4] U. S. Geol. Surv., Bulletin 179, 1902.

ORNITHOPODA

Hadrosauridæ

Hadrosaurus foulkii Leidy. Woodbury formation, near Haddonfield, Camden County.
Hadrosaurus? minor Marsh. Navesink? formation, near Barnsboro.
Hadrosaurus? cavatus Cope. Horizon and locality uncertain.
Ornithotarsus immanis Cope. Magothy? formation, near Keyport, Monmouth County.
Pneumatoarthrus peloreus Cope. Monmouth County.

No doubt better material would permit the reduction of some of these genera and species to synonymy, but for the present it is best to retain the original names as far as possible. The New Jersey dinosaurs, poorly known as they are, possess considerable importance because of their association with marine invertebrates of known stratigraphic range. It is unfortunate that the excavation of the glauconitic sands has been discontinued almost entirely, for to this activity were due most of the discoveries of New Jersey dinosaurs.

KANSAS

Most American stratigraphers consider the marine and continental sandstones and shales known as the Dakota formation to be the lowest division of the Upper Cretaceous in Kansas and adjacent states. The Dakota rocks have been referred to the Cenomanian, but Professor W. H. Twenhofel [1] regards them as part of the Lower Cretaceous. The Dakota beds are overlain by the marine sediments of the Colorado series. The lower division of the Colorado, consisting of several shale formations, is designated Benton, although this name sometimes is applied to the whole series. These lower Colorado beds carry a Turonian invertebrate fauna. The upper part of the series in Kansas consists essentially of chalk and limestone, and is called the Niobrara formation. This has yielded a rich fauna of marine vertebrates, especially mosasaurs, but only two dinosaurs of Niobrara age are known from Kansas.

[1] Geol. Surv., Kansas, Bull. 9, 1924.

LIST OF DINOSAURS FROM THE NIOBRARA FORMATION OF KANSAS

ORNITHOPODA

Hadrosauridæ

Claosaurus agilis (Marsh). Smoky Hill river.

STEGOSAURIA

Nodosauridæ

Hierosaurus sternbergii Wieland. Near Hackberry creek.

The Niobrara formation sometimes is referred to the Turonian, but it seems probable that the Benton division represents most of this stage, and that the Niobrara corresponds to the lower Senonian.

The Niobrara beds are overlain by the marine shales of the Pierre series, which are not known to have yielded determinable dinosaurian remains in Kansas.

WYOMING

In Wyoming, as in Kansas, the marine rocks of the Colorado series overlie the Dakota formation. The lower, shaly, division of the Colorado shows considerable lateral and vertical variation, due to near-shore deposition, and for the same reason the beds of this sub-series contain scattered remains of dinosaurs.

LIST OF DINOSAURS FROM THE LOWER COLORADO FORMATIONS OF WYOMING

STEGOSAURIA

Nodosauridæ

Nodosaurus textilis Marsh. Lower beds, near Como, Carbon County.

Stegopelta landerensis Williston. Horizon of the Thermopolis shale, near Lander, Fremont County.

These dinosaurs, on the basis of their stratigraphic occurrence, are of Turonian age. They are related to the stego-

saurians of the later Cretaceous, and have closer affinities with the Wealden *Polacanthus* and its allies than with the Morrison *Stegosaurus*.

The Niobrara, or upper Colorado, of Wyoming, is more shaly than calcareous, and, so far, has yielded no identifiable dinosaurs. It is overlain by the important Pierre series, which, in its purely marine facies, as developed in eastern Wyoming, is without a dinosaurian fauna. Marine invertebrates are common in this series, and indicate a Senonian age for the Pierre beds. Probably the series corresponds approximately to the Santonian and Campanian sub-stages.

The Fox Hills series succeeds the Pierre, and represents the retreat of the Upper Cretaceous sea. The Fox Hills beds contain a large fauna of marine and brackish-water invertebrates, and may be correlated with the Mæstrichtian.

The Lance formation, formerly called the Laramie, or Ceratops, beds, conformably overlies the Fox Hills series.[1] This formation is of continental deposition, and occurs in both eastern and western Wyoming. In the west the Lance beds are exposed extensively on both flanks of the Bighorn mountains, and near the eastern boundary of the state, on the west side of the Black Hills, the formation is rich in dinosaurian remains. It was in the latter area, in Niobrara County (formerly part of Converse County), that J. B. Hatcher collected the famous series of horned dinosaurs for Marsh, and here also, in more recent years, the American Museum of Natural History, and the Sternberg family, have obtained important material. The stratigraphy and lithology of the Lance formation in Niobrara County have been described by Professor R. S. Lull.[2] A few other localities for Lance dinosaurs occur in various parts of Wyoming and adjacent South Dakota.

[1] Dobbin, C. E., and J. B. Reeside, Jr., U. S. Geol. Surv., Profess. Paper 158-B, 1929.

[2] *Amer. Jour. Sci.*, Vol. 40, pp. 319–348, 1915.

LIST OF DINOSAURS FROM THE LANCE FORMATION OF WYOMING

THEROPODA

Deinodontidæ

Tyrannosaurus rex Osborn

Ornithomimidæ

Ornithomimus sedens Marsh

ORNITHOPODA

Hypsilophodontidæ

Thescelosaurus neglectus Gilmore

Hadrosauridæ

Thespesius annectens (Marsh)
Thespesius? longiceps (Marsh)

CERATOPSIA

Ceratopsidæ

Agathaumas sylvestris Cope (Black Buttes, southern Wyoming, presumably from beds of Lance age)
Triceratops horridus (Marsh)
Triceratops prorsus Marsh
Triceratops serratus Marsh
Triceratops sulcatus Marsh
Triceratops elatus Marsh
Triceratops calicornis Marsh
Triceratops flabellatus Marsh
Triceratops obtusus Marsh
Triceratops brevicornus Hatcher
Diceratops hatcheri Lull
Torosaurus latus Marsh
Torosaurus gladius Marsh

STEGOSAURIA

Nodosauridæ

Palæoscincus? latus Marsh

This fauna, as is shown below, may be used to correlate the Lance beds with other dinosaur-bearing formations of western North America. Other data, however, are necessary for

comparison with the European standard section. The fairly abundant brackish-water, and fresh-water, invertebrates belong to the locally-developed Upper Cretaceous fauna, so that the Lance formation must be correlated principally on the basis of its stratigraphic position. As noted above, the underlying Fox Hills series is apparently of Mæstrichtian age. The Lance beds, in Wyoming, are overlain by the continental Fort Union series, which, in some areas, carries Paleocene mammals. The Lance formation, therefore, is the summit of the Wyoming Upper Cretaceous, and may be correlated with the Danian.

Montana

Passing northward to the State of Montana, we find dinosaur-bearing formations present in two sections, which will be considered separately. The first of these sections occupies a large area in central and northern Montana, while the second is confined principally to Glacier County, in the northwest, and to an adjacent area in the Province of Alberta.

The first, or major section of Upper Cretaceous rocks in Montana is exposed along considerable portions of Musselshell, Missouri and Milk rivers. The succession here is relatively complete, and shows a wonderful alternation of marine and continental formations.[1] The Colorado series in this area is composed almost entirely of shales, and lacks the calcareous Niobrara division. Above these marine shales is the more or less continental Eagle formation, with a lower, transitional, member known as the Virgelle sandstone. The Eagle beds have yielded a few bones of dinosaurs, including the type of *Ornithomimus grandis* Marsh, which is referred now to *Deinodon horridus* Leidy.[2] Brackish-water and marine invertebrates of the Pierre fauna occur at some horizons, and on the basis of these and the position in the stratigraphic column, the Eagle formation may be referred to the middle Senonian.

[1] Stanton, T. W., and J. B. Hatcher, U. S. Geol. Surv., Bull. 257, 1905; Bowen, C. F., U. S. Geol. Surv., Profess. Paper 90-I, 1915.

[2] Matthew, W. D., and B. Brown, *Amer. Mus. Nat. Hist., Bull.*, Vol. 46, p. 377, 1922.

The shales of the Claggett formation overlie the Eagle sandstones, and contain a marine fauna of typical Pierre Mollusca. The Claggett beds merge, at the top, into the sandstones and shales of the Judith River formation, which is of continental deposition and rich in the remains of dinosaurs. The Judith River beds were the source of the first-known Upper Cretaceous dinosaurs from North America, the remains of which were collected by the intrepid pioneer, F. V. Hayden. Later E. D. Cope visited the "badlands" of the Judith River formation, and in subsequent years a number of palæontologists have obtained material here, but this field has never undergone the intensive collecting that has been so successful in adjacent areas.

LIST OF DINOSAURS FROM THE JUDITH RIVER FORMATION OF MONTANA

THEROPODA

Deinodontidæ

Deinodon horridus Leidy
Dromæosaurus lævifrons (Cope)

Ornithomimidæ

Ornithomimus tenuis Marsh

ORNITHOPODA

Troödontidæ

Troödon formosus Leidy

Hadrosauridæ

Trachodon mirabilis Leidy
Kritosaurus? grallipes (Cope)

CERATOPSIA

Ceratopsidæ

Monoclonius crassus Cope
Monoclonius? sphenoceras Cope
Ceratops montanus Marsh
Ceratops? recurvicornis (Cope)

STEGOSAURIA

Nodosauridæ

Palæoscincus costatus Leidy

This dinosaurian fauna is more or less ancestral to that of the Lance formation, and the palæontological evidence, therefore, agrees with that of stratigraphy, which indicates that the Judith River beds are older than the Lance. The Judith River invertebrates are brackish-water, and fresh-water, mollusks of the Pierre fauna, and on the basis of the stratigraphic position between the Claggett and Bearpaw formations, the Judith River formation is of Senonian age, and may be correlated with the upper Santonian or the lower Campanian.

The Bearpaw formation, which overlies the Judith River beds, consists almost entirely of dark grey marine shales. The rich molluscan fauna of the Bearpaw shales is made up of Pierre species, and the formation may be correlated with the upper Senonian. The series of formations from the base of the Eagle to the top of the Bearpaw is approximately the equivalent in Montana of the marine Pierre series.

The group of brackish-water and marine transition beds above the Bearpaw is known as the Lennep formation. This occupies the same relative position as the Fox Hills series to the east and south, but when traced in that direction the Lennep beds pass into an upper portion of the Bearpaw formation that is not represented in the central Montana section. Since the Upper Cretaceous sea retreated toward the southeast, this relationship of Lennep to Fox Hills is that of a regressive overlap, and the Lennep formation must be considered as somewhat older than the Fox Hills beds. A few bones of dinosaurs have been obtained from the upper beds of the Lennep formation.

The top of the Lennep is poorly defined, and merges into the overlying fresh-water beds of the Hell Creek formation. This formation has a good dinosaurian fauna, which has been studied by Mr. Barnum Brown.[1]

[1] *Geol. Soc. America, Bull.*, Vol. 25, pp. 356–359, 1914.

LIST OF DINOSAURS FROM THE HELL CREEK FORMATION OF MONTANA

THEROPODA

Deinodontidæ

Tyrannosaurus rex Osborn

Ornithomimidæ

Ornithomimus sp.

ORNITHOPODA

Hypsilophodontidæ

Thescelosaurus neglectus Gilmore

Hadrosauridæ

Thespesius annectens (Marsh)
Thespesius sp. (= "*Trachodon mirabilis*)

CERATOPSIA

Ceratopsidæ

Triceratops serratus Marsh
Triceratops brevicornus Hatcher
Triceratops sp.

STEGOSAURIA

Nodosauridæ

Palæoscincus sp.
Ankylosaurus magniventris Brown

Almost all the dinosaurs listed above also occur in the Lance formation of Wyoming, which is the stratigraphic equivalent of most of the Hell Creek beds. The latter are succeeded by the Tullock formation, which, so far, has not yielded diagnostic vertebrates. Farther east, in the adjacent part of North Dakota, the Hell Creek beds are overlain by a marine formation, called the Cannonball. This has been referred [1] to the Cretaceous on the basis of the invertebrate fauna, but other workers [2] have correlated the Cannonball

[1] Stanton, T. W., and T. W. Vaughan, U. S. Geol. Surv., Profess. Paper 128-A, 1921.

[2] Thom, W. T., and C. E. Dobbin, *Geol. Soc. America, Bull.*, Vol. 35, p. 484, 1924.

formation with the Tullock and overlying Lebo beds, the last-named containing a Middle Paleocene mammalian fauna. The writer prefers to include the Tullock and Cannonball formations provisionally in the Paleocene, regarding most of the marine invertebrates of the Cannonball as survivors of the Pierre fauna in an almost landlocked sea. According to this view, the Hell Creek formation is the uppermost unit of the Cretaceous of Montana, and, like the Lance formation, may be correlated approximately with the Danian stage.

The second Montana section to be described is situated in the northwestern portion of the state, near the eastern front of the Rocky Mountains. The more important outcrops occur in Glacier County, along St. Mary and Milk rivers. All the formations of this section may be traced northward into Alberta. The Glacier County section has been described by Dr. E. Stebinger.[1]

The Colorado shale in Glacier County is overlain by the Virgelle sandstone, which corresponds to the similarly-named lower member of the Eagle formation farther east. Above the Virgelle beds is the thick, mostly continental, Two Medicine formation. This probably is capable of further division, as the equivalent rocks just north of the International Boundary have been grouped into four distinct formations. Whether the divisions are recognized or not, the Two Medicine formation certainly includes the equivalents of the upper Eagle, the Claggett, and the Judith River formations. Dinosaurian remains have been obtained from the upper Two Medicine beds, at the horizon of the Judith River formation. This fauna has been studied by Mr. C. W. Gilmore.[2] The following list includes several species recently obtained by Mr. Gilmore, and not previously recorded from the Two Medicine formation.

[1] U. S. Geol. Surv., Profess. Paper 90-G, 1914; Bulletin 621, pp. 117–156, 1916.
[2] Smithsonian Misc. Coll., Vol. 63, No. 3, 1914; U. S. Geol. Surv., Profess. Paper 103, 1917; *Proc. U. S. Nat. Mus.*, Vol. 61, art. 3, 1922.

LIST OF DINOSAURS FROM THE TWO MEDICINE FORMATION OF MONTANA

ORNITHOPODA

Hadrosauridæ

Kritosaurus? sp. ("*Stephanosaurus marginatus*")
Hypacrosaurus? sp. ("*Hypacrosaurus altispinus?*")

CERATOPSIA

Ceratopsidæ

Brachyceratops montanensis Gilmore
Styracosaurus, sp. nov.
Monoclonius? sp.

STEGOSAURIA

Nodosauridæ

Euoplocephalus? sp.
Dyoplosaurus acutosquameus Parks
Palæoscincus, sp. nov.

As appears below, the affinities of this fauna are with the dinosaurs of the Canadian Belly River series, although the Judith River fauna probably would show closer relationships were it better known. Certainly the Two Medicine dinosaurs are of about the same age as those from the Judith River formation.

The Two Medicine formation is overlain by the Bearpaw shales, which closely resemble the rocks of the Bearpaw formation farther east. However, the Bearpaw formation of Glacier County probably does not correspond exactly to the formation of that name in central Montana. The shoreline of the Upper Cretaceous sea appears to have moved more slowly in retreat than in advance, so that the principle of regressive overlap applies particularly to the upper limits of the marine formations here considered. Hence we may regard the top of the Glacier County Bearpaw formation as somewhat older than the upper boundary of the same formation farther east.

The Bearpaw shales of Glacier County are succeeded by the Horsethief sandstone, a brackish-water, transitional, formation. It has been called Fox Hills, but if we regard the

upper limit of the Bearpaw formation as progressively older toward the northwest, we must consider the Horsethief sandstone to be even older than the Lennep formation.

Non-marine sediments predominate in the St. Mary River formation, which succeeds the Horsethief sandstone. Fresh-water Mollusca are fairly common in the St. Mary River beds, and from them, also, Mr. Barnum Brown has obtained good dinosaurian remains for the American Museum of Natural History. These dinosaurs await description, but a partial skeleton of *Leptoceratops gracilis* Brown is known to be present. This genus and species indicates that the St. Mary River formation, in part, at least, is the equivalent of the Edmonton formation of Alberta. The St. Mary River beds are succeeded by the Willow Creek formation, which may be correlated stratigraphically with Paleocene beds in Alberta. As an unconformity has not been observed at the base of the Willow Creek formation, we may consider the equivalents of both the Fox Hills (Edmonton) and Lance beds to be present in the St. Mary River formation.

Alberta

It will be most convenient to consider next the Upper Cretaceous section in the southern part of the Province of Alberta. The pioneer geological studies in this area were made by G. M. Dawson and Mr. J. B. Tyrrell. Most of the formations are described in the late Dr. D. B. Dowling's monograph, "The Southern Plains of Alberta." [1] The dinosaurs of Alberta have been described in a series of papers by the late Mr. L. M. Lambe, and by Mr. Barnum Brown.

The Upper Cretaceous formations of southern Alberta continue southward into both the central Montana and Glacier County sections. The lowest formation exposed on the southern Alberta plains is the Milk River sandstone, which is of non-marine deposition, and corresponds to the Eagle formation of Montana. Above the Milk River beds is the marine Pakowki shale, approximately the equivalent of the

[1] Geol. Surv. Canada, Memoir 93, 1917.

Claggett formation. The Pakowki shale is succeeded by the Foremost beds, principally soft, brownish, sandstones, which are of brackish-water deposition, and represent the transition from the marine conditions of Pakowki time to the purely fresh-water conditions represented by the Pale beds. Remains of an unidentifiable hadrosaur have been collected from the Foremost beds. This member may be traced into the lower part of the Judith River formation.

The next stratigraphic unit, in ascending order, is called the Pale beds, an objectionable name that survives from Dawson's nomenclature. The rocks of this member are predominantly soft, light-colored, sandstones, of fresh-water deposition except near the top. The Pale beds are extensively exposed in the "badlands" of the Red Deer river, near Steveville, where large-scale collecting has brought to light one of the richest of known dinosaurian faunas.

LIST OF DINOSAURS FROM THE PALE BEDS OF ALBERTA

THEROPODA

Deinodontidæ

Deinodon horridus Leidy?
Gorgosaurus libratus Lambe
Gorgosaurus sternbergi Matthew and Brown
Dromæosaurus albertensis Matthew and Brown

Cœluridæ

Chirostenotes pergracilis Gilmore

Ornithomimidæ

Ornithomimus altus Lambe (*Struthiomimus altus* Osborn)
Ornithomimus samueli (Parks)

ORNITHOPODA

Troödontidæ

Troödon validus (Lambe)

Hadrosauridæ

Kritosaurus marginatus (Lambe)
Kritosaurus notabilis (Lambe)
Kritosaurus incurvimanus Parks

Prosaurolophus maximus Brown
Lambeosaurus lambei Parks
Corythosaurus casuarius Brown
Corythosaurus excavatus Gilmore
Corythosaurus intermedius Parks
Corythosaurus? selwyni (Lambe)
Parasaurolophus walkeri Parks
Didanodon altidens (Lambe) (? = "*Procheneosaurus*")

CERATOPSIA

Ceratopsidæ

Eoceratops canadensis (Lambe)
Brachyceratops? dawsoni (Lambe)
Centrosaurus apertus Lambe
Centrosaurus nasicornus (Brown)
Centrosaurus? cutleri (Brown)
Styracosaurus albertensis Lambe
Chasmosaurus belli (Lambe)

STEGOSAURIA

Nodosauridæ

Palæoscincus costatus Leidy
Palæoscincus asper Lambe
Euoplocephalus tutus (Lambe)
Panoplosaurus mirus Lambe
Dyoplosaurus acutosquameus Parks

This fauna is of approximately the same age as that of the Judith River formation, and no doubt many of the differences would disappear if the Judith River dinosaurs were better known.

The group of formations from the base of the Milk River to the top of the Pale beds constitutes the Belly River series of Dawson. The dinosaurs of the Pale beds usually are cited as from the "Belly River formation." Recently some stratigraphers have limited the application of the term Belly River series to the Foremost and Pale beds. This usage seriously impairs the value of the name Belly River, and reduces it to a mere synonym of Judith River formation.

The upper boundary of the Pale beds is fairly sharp, and above it are the dark grey marine shales of the Bearpaw

formation. In western Alberta this is the equivalent of the similarly-named formation in Glacier County, Montana.

The Horsethief sandstone continues into Alberta as a distinct lithological unit for about 40 miles, after which its place is taken by a transition series of brackish-water beds in the basal portion of the overlying continental formation.

In the southwestern portion of Alberta, for about 60 miles north of the International Boundary, the Horsethief or Bearpaw beds are overlain by the sandstones and shales of the St. Mary River formation, which are almost entirely of fresh-water deposition. In Alberta this formation has furnished only a few dinosaur bones, but, like the St. Mary River beds in Montana, appears to be the equivalent of the Edmonton and Lance formations.

Farther north in Alberta the formation above the Bearpaw beds is the Edmonton, which is similar in lithology to the Pale beds, and, like them, is exposed in "badlands" along Red Deer river, where a rich dinosaurian fauna has been collected.

LIST OF DINOSAURUS FROM THE EDMONTON FORMATION OF ALBERTA

THEROPODA

Deinodontidæ

Albertosaurus sarcophagus Osborn
Albertosaurus arctunguis Parks
Dromæosaurus? sp.

Cœluridæ

Chirostenotes? sp.

Ornithomimidæ

Ornithomimus brevitertius Parks
Ornithomimipus angustus Sternberg (footprints)

ORNITHOPODA

Hypsilophodontidæ

Thescelosaurus sp.
Thescelosaurus? warreni Parks

Troödontidæ

Troödon sp.

Hadrosauridæ

Thespesius edmontoni Gilmore
Edmontosaurus regalis Lambe
Saurolophus osborni Brown
Hypacrosaurus altispinus Brown
Cheneosaurus tolmanensis Lambe

CERATOPSIA

Ceratopsidæ

Anchiceratops ornatus Brown
Anchiceratops longirostris Sternberg
Arrhinoceratops brachyops Parks
Leptoceratops gracilis Brown

STEGOSAURIA

Nodosauridæ

Ankylosaurus magniventris Brown
Edmontonia longiceps Sternberg
Anodontosaurus lambei Sternberg

This list contains few genera that occur in the Pale beds or the Judith River formation. Several characteristic Lance genera (*Thespesius*, *Thescelosaurus*, *Ankylosaurus*) are present in the Edmonton fauna, but some of these, at least, are confined to the higher beds of the formation. Near the base and at about the middle the Edmonton formation contains brackish-water invertebrates of Fox Hills type. As the Bearpaw formation of Alberta is at least as old as that of central Montana, and probably does not include representatives of the uppermost Pierre beds of Wyoming and South Dakota, it appears likely that the Edmonton formation is the Alberta equivalent of both the Lennep and Fox Hills formations. It is also possible that some of the upper Edmonton beds are of Lance age, but as the upper boundary of the formation, in the area where dinosaurs are abundant, is an erosion surface, Lance equivalents are likely to be present only as irregular remnants.

Saskatchewan

The stratigraphy of the younger beds in Saskatchewan was studied by R. G. McConnell,[1] but the formations in the southern part of the province have been described more recently in a number of publications of the Canadian Department of Mines.[2]

The Bearpaw shales underlie much of southern Saskatchewan, and correspond approximately to the Bearpaw formation of central Montana. Above the Bearpaw the beds are predominantly of non-marine deposition, and have been difficult to interpret because of lateral variation. As the section is now understood, the first of these post-Bearpaw units is a variable group of sandstones, which has been called Fox Hills, but which Dr. McLearn has designated Basal sandstone. This has yielded some fragmentary dinosaur bones, and, at the uppermost level, a vertebra of *Triceratops* sp. It is possible, however, that the horizon of the last-mentioned fossil actually is part of the overlying Ravenscrag formation. The Basal sandstone probably should be correlated with the Lennep formation, or with some lower portion of the Fox Hills series.

In many places the Basal sandstone is overlain by the Whitemud beds, which are composed of light-colored, clayey, sandstones. When present these beds form a conspicuous horizon, but unfortunately they are often absent, presumably as a result of an interval of erosion. Furthermore, the presence of beds resembling the Whitemud at higher horizons in the section has confused some stratigraphers. The typical Whitemud beds have not yielded fossils, but may be compared with some upper members of the Fox Hills series in central and eastern Montana.

The Ravenscrag formation unconformably overlies the Whitemud beds, or, where they are absent, rests upon the

[1] Geol. Surv. Canada, Ann. Rept., n.s., Vol. 1, pt. C, 1886.

[2] Rose, B., Geol. Surv. Canada, Mem. 89, 1916; Davis, N. B., Canada, Dept. of Mines, Mines Branch, Publicat. No. 468, 1918; McLearn, F. H., Geol. Surv. Canada, Summary Rept. for 1927, pt. B, pp. 21–53, 1928; Summ. Rept. for 1928, pt. B, pp. 30–45, 1929.

Basal sandstone. The Ravenscrag beds are coal-bearing, and may be divided into lower and upper portions at the level of the lowest seam. The lower Ravenscrag beds have yielded a number of dinosaurian fossils. In southwestern Saskatchewan, near the Cypress Hills, remains of *Triceratops* sp. occur at various horizons, including the basal members. Farther east, near Wood Mountain, G. M. Dawson, in 1874, collected vertebrate fossils that were identified by E. D. Cope. The recent work of Mr. C. M. Sternberg[1] supplies an adequate list of lower Ravenscrag vertebrates.

LIST OF DINOSAURS FROM THE LOWER RAVENSCRAG BEDS OF SASKATCHEWAN

THEROPODA

Deinodontidæ

Gen. and sp. undet.

Ornithomimidæ

Ornithomimus sp.

ORNITHOPODA

Hypsilophodontidæ

Thescelosaurus neglectus Gilmore

Hadrosauridæ

Thespesius saskatchewanensis Sternberg

CERATOPSIA

Ceratopsidæ

Triceratops prorsus Marsh?
Triceratops sp.

On the basis of this fauna the lower Ravenscrag beds may be correlated definitely with the Hell Creek beds of Montana, and the Lance formation of Wyoming.

The upper part of the Ravenscrag formation, overlying the lowest coal seam, has not yielded diagnostic vertebrates. The none-marine invertebrates present here suggest the Fort

[1] *Canadian Field-Naturalist*, Vol. 38, pp. 66–70, 1924.

Union fauna, and the upper Ravenscrag beds presumably are of Paleocene age.

Colorado

The Upper Cretaceous sediments lying east of the Rocky Mountains near Denver, Colorado, are involved deeply in the "Laramie" controversy of American geology. The stratigraphy of this area was studied some years ago by the United States Geological Survey.[1]

The marine sandstones called Fox Hills in Colorado, apparently corresponding to the upper portion of the true Fox Hills series, are overlain by the continental, coal-bearing, beds of the Laramie formation. This is the only group of beds to which the name Laramie should be applied. Dinosaurian remains from this formation include bones referred by Marsh to the Ornithopoda, and the more recently discovered horn cores of *Triceratops?* sp. The presence of the latter fossils, as well as the stratigraphic position, suggests a Lance age for the Laramie formation.

The beds above the Laramie formation are mostly composed of conglomerate, and make up the Arapahoe formation. This has yielded a small dinosaurian fauna.

LIST OF DINOSAURS FROM THE ARAPAHOE FORMATION OF COLORADO

Ornithopoda

Hadrosauridæ

Thespesius? sp.

Ceratopsia

Ceratopsidæ

Triceratops spp.

This short list suggests the Lance fauna. The Arapahoe conglomerates were thought, at one time, to indicate a long interval of erosion, but this view is being abandoned.[2] We

[1] U. S. Geol Surv., Monograph 27, 1896.

[2] Dobbin, C. E., and J. B. Reeside, Jr., U. S. Geol. Surv., Profess. Paper 158-B, pp. 23, 24, 1929.

may correlate the Arapahoe formation, like the underlying Laramie beds, with the Lance of Wyoming.

The Arapahoe beds pass upward into the finer sediments of the Denver formation, which contains a fair dinosaurian fauna.

LIST OF DINOSAURS FROM THE DENVER FORMATION OF COLORADO

THEROPODA

Ornithomimidæ

Ornithomimus velox Marsh

ORNITHOPODA

Hadrosauridæ

Thespesius? sp.

CERATOPSIA

Ceratopsidæ

Triceratops alticornis (Marsh)
Triceratops horridus (Marsh)?

This fauna is a typical Lance assemblage. Messrs. Thom and Dobbin [1] regard the Denver formation as the equivalent of the Lance and possibly some younger beds.

NEW MEXICO

Dinosaur-bearing beds in San Juan County, northwestern New Mexico, were described first by Mr. Barnum Brown.[2] Subsequent, more detailed, studies were published by the United States Geological Survey.[3]

The Pierre series apparently is represented in San Juan County by the upper part of the Mancos shale and the Mesaverde, Lewis and Pictured Cliffs formations. The continental, coal-bearing, Fruitland formation rests conformably upon the Pictured Cliffs sandstone. The large

[1] *Geol. Soc. Amer., Bull.*, Vol. 35, pp. 498, 499, table 2, 1924.

[2] *Amer. Mus. Nat. Hist., Bull.*, Vol. 28, art. 24, 1910.

[3] U. S. Geol. Surv., Profess. Paper 98, pp. 271–353, 1916; Profess. Paper 119, 1919; Profess. Paper 134, 1924.

invertebrate fauna of the Fruitland beds has been used to correlate them with the Fox Hills series. Important dinosaurian remains also are present.

LIST OF DINOSAURS FROM THE FRUITLAND FORMATION OF NEW MEXICO

THEROPODA

Deinodontidæ

Gen. and sp. undet.

CERATOPSIA

Ceratopsidæ

Pentaceratops sternbergii Osborn

The next formation, in ascending order, is the Kirtland shale, the vertebrate fauna of which has been listed by Mr. Gilmore.

LIST OF DINOSAURS FROM THE KIRTLAND FORMATION OF NEW MEXICO

THEROPODA

Deinodontidæ

Gen. and sp. undet.

ORNITHOPODA

Hadrosauridæ

Kritosaurus navajovius Brown

CERATOPSIA

Ceratopsidæ

Gen. and sp. undet.

STEGOSAURIA

Nodosauridæ

Gen. and sp. undet.

The Ojo Alamo sandstone overlies the Kirtland shale, and also contains a dinosaurian fauna.

LIST OF DINOSAURS FROM THE OJO ALAMO FORMATION OF NEW MEXICO

SAUROPODA

Atlantosauridæ

Alamosaurus sanjuanensis Gilmore

THEROPODA

Deinodontidæ

Deinodon? sp.

ORNITHOPODA

Hadrosauridæ

Kritosaurus navajovius Brown

CERATOPSIA

Ceratopsidæ

Monoclonius? sp.

STEGOSAURIA

Nodosauridæ

Gen. and sp. undet.

The dinosaurian faunas of the Fruitland, Kirtland, and Ojo Alamo formations may be considered, for the present, as a single fauna. This has been regarded by both Brown and Gilmore as approximately the equivalent of the Judith River and Belly River faunas. However, the invertebrate evidence indicates a Fox Hills age for the Fruitland formation, placing it in approximately the same stage as the Edmonton formation of Alberta. The overlying Kirtland and Ojo Alamo formations must be at least as young, and the combined dinosaurian faunas may be correlated provisionally with that of the Edmonton formation. It should be noted in this connection that *Pentaceratops* is very like *Arrhinoceratops* of the Edmonton fauna, and that it is still somewhat uncertain that the Belly River species referred to *Kritosaurus* actually are congeneric with *Kritosaurus navajovius*. Further work on these New Mexican dinosaurs, now in progress at the U. S. National Museum, may permit more definite correlation.

Conclusions

The dinosaur-bearing formations of the North American Upper Cretaceous may be grouped stratigraphically into four rather distinct stages. The lowest of these is made up of the Colorado series, containing a poorly-known fauna from widely scattered localities. Possibly more complete dinosaurian faunas from the Colorado series would indicate the presence of two stages, corresponding to the Benton, and the Niobrara, respectively. This Colorado dinosaurian stage is of Turonian and lower Senonian age.

The second stage may be called Pierre, as most of the dinosaurian faunas of this stage occur in continental beds corresponding to the upper part of the Pierre series. This Pierre stage is correlated with the Senonian, either Santonian or Campanian, and includes the dinosaurian faunas of the New Jersey Cretaceous from Magothy to Navesink, the Judith River and Two Medicine formations, and the Pale beds of the Belly River series.

The third, or Fox Hills, stage, corresponds approximately to the Fox Hills series, and includes the dinosaurs of the Edmonton and St. Mary River formations. Here, also, may be placed the faunas of the Fruitland, Kirtland, and Ojo Alamo formations. The Fox Hills stage is correlated with the Mæstrichtian.

Finally, we have the Lance stage, the last in the dinosaurian succession of North America. To this stage may be referred the dinosaurs of the Lance, Hell Creek, Ravenscrag, Laramie, Arapahoe, and Denver formations. These are all presumably of Danian age.

It might be preferable to designate these stages by the names of characteristic genera of dinosaurs. However, only the last, the Lance stage, contains a genus, *i.e.*, *Triceratops*, that is sufficiently widespread to be characteristic of the stage. The Lance stage, therefore, might be known also as the *Triceratops* stage.

The succession of faunas described above must be kept in mind when dealing with the North American Upper Cre-

taceous dinosaurs. Any generalization that opposes the stratigraphic evidence is open to question, no matter how strong the apparent support of morphology. The writer will cite only one example of this, although others might be found. Baron Francis Nopcsa, in a recent paper entitled, "Sexual Differences in Ornithopodous Dinosaurs,"[1] attempts to recognize the corresponding males and females among the various genera and species of Ornithopoda. A comparison of Nopcsa's conclusions with the faunal lists given above shows that the supposed males and females of single species occur, in most cases, in separate geological formations of distinctly different ages.

[1] *Palæobiologica*, Vol. 2, pp. 187–201, 1929.

PARALLEL CONNECTIONS OF IDENTICAL NETS

By A. E. KENNELLY

(Read April 24, 1930)

Object of the Enquiry.—It is sometimes required to connect a group of identical electrical networks in parallel. If the constants of any single such net are known, for the particular frequency of operation considered, the question arises as to the corresponding constants of the group regarded as a single resultant net.

The constants of a net for a given alternating-current impressed frequency, are, in general, three. These may be taken either as the net angle θ, the geomean surge impedance z_{ab} and the inequality factor q; or the net angle θ, the two end surge impedances $z_{0a}z_{0b}$; or the three elements of the equivalent T, $\rho_1\rho_2$ and R or the elements of the equivalent Π, g_1g_2 and ρ. Any one such set of three determining net constants may be converted into any other set, through the use of known formulas.

Two Identical Nets in Direct Parallel Connection.—Let $AGBH$, Fig. 1, be any dissymmetrical four-terminal net, with input terminals AG and output terminals BH. At the frequency of operation, let the angle of the net be θ hyps. $\angle$, its geomean surge impedance be z_{ab} ohms $\angle$ and its inequality factor be q. Let it be required to find the corresponding constants of the net formed by connecting two such identical nets in parallel with corresponding terminals united, as well as the equivalent T and Π of the same.

We may take any pair of identical nets such as the reactanceless pair of T's and Π's in Figs. 2 and 3, placing them in direct parallel connection; *i.e.* with A'' connected to A', and B'' connected to B'. The resultant net *agbh* of Figs. 2A and 3A will have the same angle θ and the same inequality factor q as each of the two components; but the geomean surge

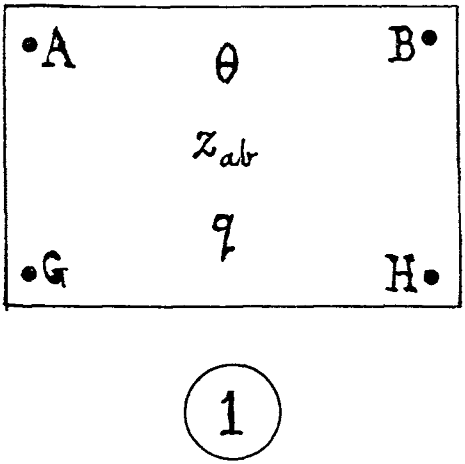
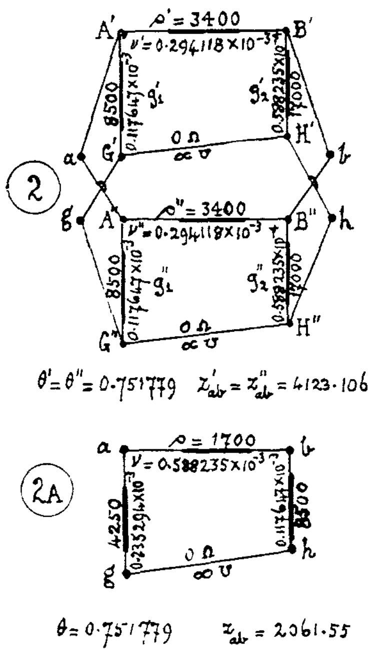


FIG. 1.—General Four-Terminal Net *AGBH*.



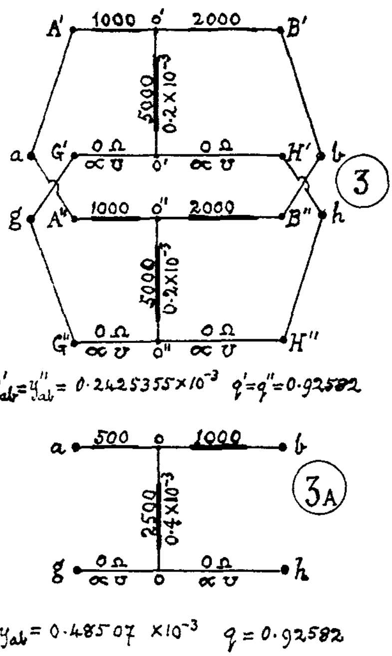


FIG. 2.—Π-Connection. FIG. 3.—*T*-Connection.

Two identical Nets $A'G'B'H'$ and $A''G''B''H''$ in direct parallel connection to form a resultant net *agbh*.

admittance y_{ab} of the resultant net will be double that of either of the two components. This is because the resultant architrave *ab*, and the resultant pillars *ag* and *bh* have all twice the admittance of their respective components. Hence, if θ', θ'' and θ are the two component net angles and the resultant net angle respectively,

$$\theta = \theta' = \theta'' \qquad \text{hyps} \angle \quad (1)$$

while similarly, for the inequality coefficients:

$$q = q' = q'' \qquad \text{numeric} \angle \quad (2)$$

while for the surge admittances,

$$y_{ab} = 2y_{ab}' = 2y_{ab}'' \qquad \text{mhos} \angle \quad (3)$$

or, in terms of the surge admittances

$$z_{ab} = \tfrac{1}{2}z_{ab}' = \tfrac{1}{2}z_{ab}'' \qquad \text{ohms} \angle \quad (4)$$

Any number of identical Dissymmetrical Nets in Direct Parallel Connection.—Extending the above reasoning to n identical nets in direct parallel connection, the resulting net has the same angle θ as each component, and also the same inequality factor q; but its geomean surge admittance y_{ab} is n times that of each component. That is:

$$\theta = \theta' = \theta'' = \theta''' \text{ etc.} \qquad \text{hyps} \angle \quad (5)$$

and

$$q = q' = q'' = q''' \text{ etc.} \qquad \text{numeric} \angle \quad (6)$$

but

$$y_{ab} = ny_{ab}' = ny_{ab}'' \text{ etc.} \qquad \text{mhos} \angle \quad (7)$$

or

$$z_{ab} = z_{ab}'/n = z_{ab}''/n \text{ etc.} \qquad \text{ohms} \angle \quad (8)$$

In this proposition concerning the direct parallel connection of n identical nets, it should be noted that the identity required extends to all four elements of the individual nets, regarded as equivalent Π's or *O*'s, and to all five elements of the same, regarded as equivalent *T*'s or *I*'s. Thus, if as in Fig. 4, all of the series impedance ρ should be inserted in the upper element *A'B'* of one component net, and in the lower

element $G''H''$ of the other, the resultant net *agbh* would have zero resistance in both upper and lower elements; so that its angle θ would be reduced to zero. It is evident therefore, that the identical component nets should be regarded as O's and I's, rather than as Π's and T's, for the purpose of parallel connection; whereas for series connection, this distinction might be unnecessary.

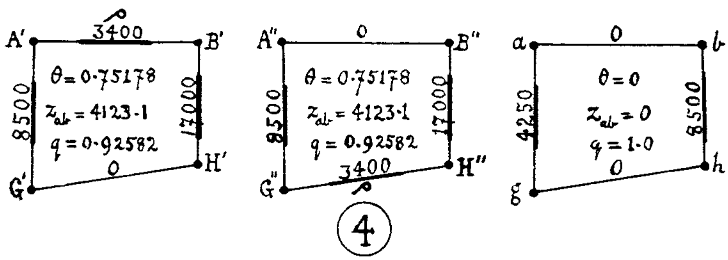


FIG. 4.—Two component nets of $\theta = 0.75178$ hyp. producing in direct parallel connection the resultant net *agbh* of $\theta = 0$.

If then a net has, at a given frequency, an angle θ, inequality factor q, with surge admittance y_{ab}, and it is desired to transform it into a net of the same θ and q; but with surge admittance ny_{ab}, where n is an integer, it will be sufficient to place n such nets in direct parallel connection, and the resultant net will satisfy the requirement. Coincidently, the admittances of the various elements in the resultant O and I will all be increased n times.

Again, if a direct parallel group of n identical nets offers in the resultant net the three characteristics θ, q and ny_{ab}, a change from n to n' components in the group will change these characteristics to θ, q, and $n'y_{ab}$.

Two identical Dissymmetrical Nets connected in Reverse Parallel.—Fig. 5 represents two identical nets $A'G'B'H'$ and $A''G''B''H''$, in equivalent O form, connected in reverse parallel between the resultant terminals *ag* and *bh*. The resultant O is indicated at *agbh*, Fig. 6 and the resultant I at $a'g'b'h'$. The corresponding Π and T equivalent nets are shown in Fig. 7.

It will be seen that the resultant O and I of Fig. 6 are symmetrical, and the same as would be obtained from the parallel connection of the symmetrical O's of Fig. 8, in which the two leaks AG and BH are equal, each being the arith-

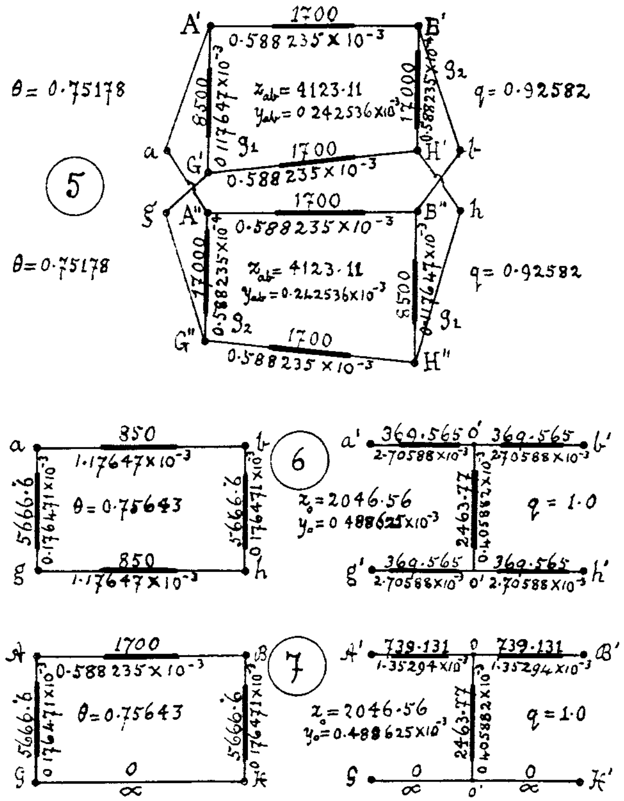


FIG. 5.—Two identical nets $A'B'G'H'$ and $A''B''G''H''$ in reverse parallel connection.
FIG. 6.—Symmetrical resultant O and I.
FIG. 7.—Equivalent symmetrical Π and T.

metical mean conductance of the two unequal leaks in Fig. 5. The corresponding I and T equivalents of the mean-leak symmetrical O in Fig. 8. appear in Figs. 9 and 11.

Consequently, if two identical dissymmetrical nets are connected in reverse parallel, the combination is equivalent to the simple parallel connection of two identical symmetrical

nets, in each of which the leaks have the arithmetical mean admittance of the leaks in the components.

O's or Π's symmetrised by equal mean admittance Leaks.—It is shown in the Appendix, that if θ, y_{ab} and q are the characteristics of a dissymmetrical O net, θ', y_0' and $q' = 1$, those of the corresponding O net symmetrised with equal total leak admittance, the following relations hold:

$$\cosh \theta' = \cosh \theta \times \left(\frac{q + q^{-1}}{2}\right) \quad \text{numeric } \angle \qquad (9)$$

$$\frac{y_0'}{y_{ab}} = \frac{\sinh \theta'}{\sinh \theta} \qquad \text{numeric } \angle \qquad (10)$$

In the case of the dissymmetrical O in Fig. 5, $A'G'B'H'$, hyp. with $q = 0.92582$ and $q^{-1} = 1.08012$; so that $\cosh \theta = 1.29615$ and $\frac{q + q^{-1}}{2} = \frac{2.00594}{2} = 1.00297$. Thus $\cosh \theta' = 1.29615 \times 1.00297 = 1.3000$ and $\theta' = 0.75643$, which is the angle of the mean-leak symmetrised O in Fig. 8, $AGBH$; or the equivalent Π of the same $agbh$, Fig. 10.

Again, the surge admittance y_{ab} of the dissymmetrical $O - A'G'B'H'$ is 0.242536×10^{-3}, corresponding to a surge impedance z_{ab} of 4123.11 ohms; also $\sinh \theta = 0.824620$ and $\sinh \theta' = 0.830662$; so that

$$y_0 = 0.242536 \times 10^{-3} \times \frac{0.830662}{0.824620}$$
$$= 0.242536 \times 10^{-3} \times 1.00733 = 0.244313 \times 10^{-3}$$

or $z_0 = 4093.12$ ohms.

Symmetrising the O of Fig. 5 for the same total leak admittance has therefore increased the net angle θ from 0.75178 to 0.75643 hyps. and reduced the geomean surge impedance from 4123.11 to 4093.12 ohms.

Auxiliary Angle for the Computation of $(q + q^{-1})/2$.—It may be noted that the factor $(q + q^{-1})/2$ in (9) can sometimes be computed more easily with the aid of an auxiliary hyperbolic angle, especially in alternating-current nets, where q and

q^{-1} are complex numbers. Thus, let

$$q = \tanh \vartheta \qquad \text{numeric } \angle \qquad (11)$$

where ϑ is an auxiliary hyperbolic angle, in general complex. Then

$$\frac{q + q^{-1}}{2} = \frac{\tanh \vartheta + \coth \vartheta}{2} = \coth 2\vartheta \quad \text{numeric } \angle \qquad (12)$$

In the case of Fig. 5, with $q = 0.92582$, $\vartheta = 1.6283$ and $2\vartheta = 3.2566$; whence $\coth 2\vartheta = 1.00297$.

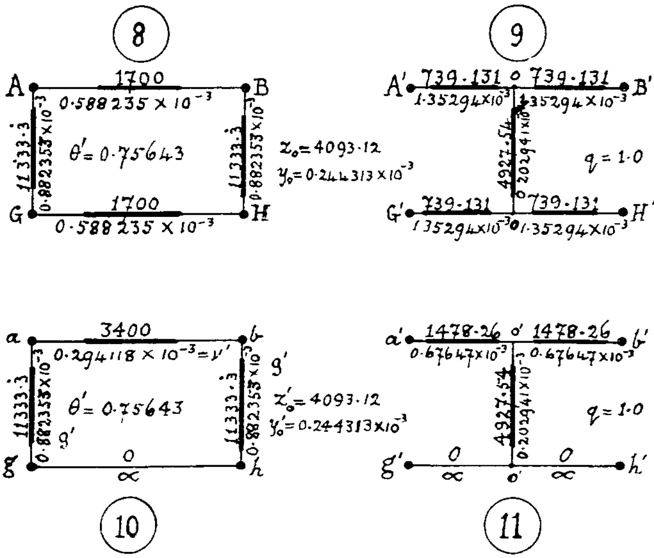


FIG. 8.—Mean-leak symmetrical O of net $A'G'B'H'$ in Fig. 5.
FIG. 9.—Corresponding symmetrical I.
FIG. 10.—Mean-leak symmetrical Π of net $A'G'B'H'$ in Fig. 5.
FIG. 11.—Corresponding symmetrical T.

Figs. 9 and 11 show the I and T equivalents corresponding to the symmetrical mean-leak O and Π of Figs. 8 and 10.

Having obtained the mean-leak symmetrical O and Π of Figs. 8 and 10, the resultant O of two such nets in simple parallel is that given in Fig. 6, and represents the resultant of the two dissymmetrical nets of Fig. 5 in reverse parallel connection.

Appendix

Symmetrisation of a dissymmetrical Net presented in the Form of an O or Π by equalising the leak Admittances.—If any dissymmetrical net is presented in equivalent O form, as at $A'G'B'H'$, Fig. 5, with leaks of unequal admittance, it can evidently be converted into an equivalent Π as at $A'G'B'H'$, Fig. 2, by placing all of the series impedance in the architrave $A'B'$. The net characteristics θ, z_{ab} and q will clearly be the same for this O and Π.

If the Π of $A'G'B'H'$ of Fig. 2 is then *symmetrised* or reduced to symmetry, at *agbh*, Fig. 10, by placing in each leak g' the vector arithmetical mean of the leaks g_1 and g_2 in Figs. 2 and 5, we have:

$$g' = \frac{g_1' + g_2'}{2} \qquad \text{mhos } \angle \qquad (13)$$

Then in the symmetrised Π of Fig. 10, as in any symmetrical Π,

$$\frac{\frac{1}{2}(g_1 + g_2)}{\nu'} + 1 = \frac{g'}{\nu'} + 1 = \cosh \theta' \quad \text{numeric } \angle \qquad (14)$$

In the original dissymmetrical net $A'G'B'H'$ of Fig. 2, the following general relation is known to hold:

$$\frac{\frac{1}{2}(g_1 + g_2)}{\nu} + 1 = \cosh \theta \times \left(\frac{q + q^{-1}}{2}\right) \quad \text{numeric } \angle \qquad (15)$$

hence

$$\cosh \theta' = \cosh \theta \times \left(\frac{q + q^{-1}}{2}\right) \quad \text{numeric } \angle \qquad (16)$$

or the cosine of the symmetrised Π angle is equal to the cosine of the dissymmetrical Π angle multiplied by the factor $(q + q^{-1})/2$. From this relation, the angle θ' of the symmetrised Π or O can be found.

The inequality factor q of the symmetrised Π is manifestly $1.0\underline{/0^\circ}$ which means that the new surge impedance $z_{ab} = z_0$ and is symmetrical.

To find z_0, it may be observed that in any dissymmetrical Π, the architrave impedance ρ is always equal to z_{ab} sinh θ,

whatever may be the value of the inequality factor q. But in converting the dissymmetrical Π, $A'G'B'H'$ of Fig. 2 into the mean-leak symmetrical Π of Fig. 10, the architrave impedance ρ has been left unchanged. Hence:

$$z_0 \sinh \theta' = z_{ab} \sinh \theta \qquad \text{ohms } \angle \quad (17)$$

or

$$\frac{z_0}{z_{ab}} = \frac{\sinh \theta}{\sinh \theta'} \qquad \text{numeric } \angle \quad (18)$$

or

$$\frac{y_0}{y_{ab}} = \frac{\sinh \theta'}{\sinh \theta} \qquad \text{numeric } \angle \quad (19)$$

It may be noted that the last three equations apply to any transformation affecting Π leaks, in which the architrave impedance remains unchanged, as for example, in the theory of uniform leak-loaded lines.*

Symmetrisation of a Dissymmetrical Net presented as a T or I, by equalising the Series Arms.—If any dissymmetrical net is presented in equivalent T form, as at $AGBH$, Fig. 12, it can be converted into the corresponding T form, as at $A'G'B'H'$, Fig. 13, by placing all of the series impedance in the upper arms $A'O'$ and $O'B'$. The net characteristics will evidently be the same for both I and T.

If the dissymmetrical T of Fig. 13 is symmetrised by equalising arms; *i.e.* by placing in each arm an impedance ρ equal to the arithmetical mean of the original arm impedances; so that

$$\rho = \frac{\rho_1 + \rho_2}{2}. \qquad \text{ohms } \angle \quad (20)$$

Then in the dissymmetrical T the following general relation holds:

$$\frac{\frac{1}{2}(\rho_1 + \rho_2)}{R} + 1 = \cosh \theta \times \left(\frac{q + q^{-1}}{2}\right) \text{numeric } \angle \quad (21)$$

while in the symmetrised T,

$$\frac{\frac{1}{2}(\rho_1 + \rho_2)}{R} + 1 = \frac{\rho}{R} + 1 = \cosh \theta' \quad \text{numeric } \angle \quad (22)$$

* Bibliography 8.

so that

$$\cosh \theta' = \cosh \theta \times \left(\frac{q + q^{-1}}{2}\right) \quad \text{numeric} \angle \qquad (23)$$

which is the same relation as (16).

Consequently, when any dissymmetrical net is presented in the form of its equivalent Π and is then symmetrised for mean leaks, its new angle θ' will be the same as when presented in the form of its equivalent T and then symmetrised for mean arm impedance.

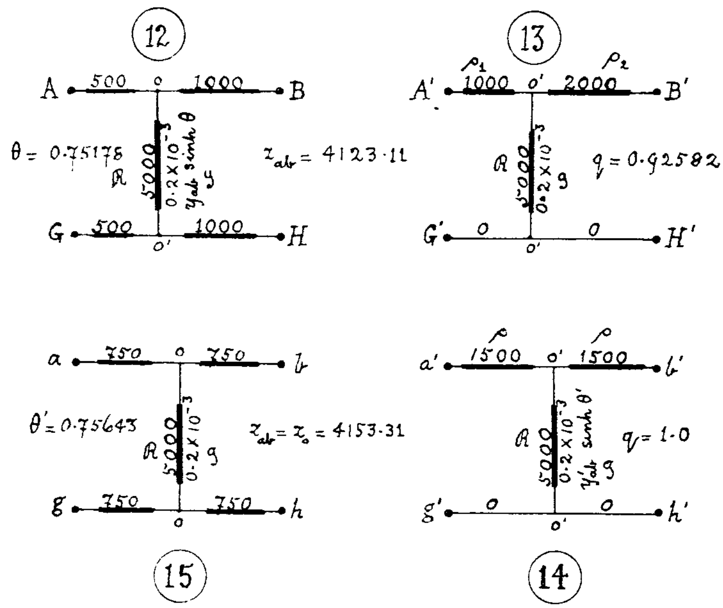


FIG. 12.—Dissymmetrical balanced I corresponding to T of Fig. 3A.
FIG. 13.—Dissymmetrical T as derived from Fig. 12.
FIG. 14.—Mean-arm symmetrical T derived from Fig. 13.
FIG. 15.—Mean-arm symmetrical balanced I derived from Fig. 14.

The inequality factor q of the symmetrised T is manifestly $1.0 \angle 0°$, which means that $z_{ab}' = z_0$, a symmetrised surge impedance.

To find z_0, it may be observed that in any dissymmetrical T, the staff admittance is always $y_{ab} \sinh \theta$, whatever the value of q may be. But in symmetrising the T, $A'G'B'H'$ of Fig. 13,

into the mean-impedance arm T, $a'g'b'h'$ of Fig. 14, the staff impedance R or admittance g has been left unchanged. Hence:

$$y_0 \sinh \theta' = y_{ab} \sinh \theta \qquad \text{mhos } \angle \qquad (24)$$

or

$$\frac{y_0}{y_{ab}} = \frac{\sinh \theta}{\sinh \theta'} \qquad \text{numeric } \angle \qquad (25)$$

or

$$\frac{z_0}{z_{ab}} = \frac{\sinh \theta'}{\sinh \theta} \qquad \text{numeric } \angle \qquad (26)$$

It will be evident that the last three formulas apply to any transformation affecting T arms, in which the staff admittance remains unchanged, as for example in the theory of series-loaded lines.*

Comparing (19) with (26), we see that a net symmetrised through its Π leaks has the same angle θ' as when symmetrised through its T arms. The Π-surge impedance z_{0p} is, however, different from the T-surge impedance z_{0t}. Nevertheless, the geomean value of the two equalised surge impedances is the same as the geomean surge impedance of the original dissymmetrical net. That is

$$\sqrt{z_{0p} \cdot z_{0t}} = z_{ab} \qquad \text{ohms} \qquad (27)$$

Summary of Results.—1. Whereas the result of connecting identical symmetrical nets in simple series, is to increase the resulting net angle, keeping the surge impedance constant, the result of connecting identical symmetrical nets in simple parallel, is to keep the resulting net angle constant; while increasing the surge admittance.

2. If n identical nets of θ, y_{ab} and q, are placed in direct parallel connection, the resulting net will have the same θ, and q; but the surge admittance will be increased to ny_{ab}.

3. If a pair of identical dissymmetrical nets are placed in reverse parallel connection, the resulting net will be symmetrical, and will be equivalent to a pair of the component nets, each symmetrised with mean leaks.

* Bibliography 8.

4. Any dissymmetrical net presented in the form of an equivalent Π or O, can be symmetrised with mean leaks. Its angle's sine will then be changed and likewise its surge impedance z_0; but their product will remain equal to the original product z_{ab} sinh θ.

5. Any dissymmetrical net presented in the form of an equivalent T or I, can be symmetrised with mean series arms. Its angle's sine and the surge admittance y_0 will be changed; but their product sinh $\theta' \cdot y_0$ will remain equal to the original product sinh $\theta \cdot y_{ab}$.

6. Identical nets connected in series may often be more conveniently dealt with numerically when presented in T or I form; whereas identical nets connected in parallel may often be more conveniently dealt with when presented in Π or O form.

7. A dissymmetrical net may be given mean symmetry either through its equivalent T or through its equivalent Π. The former is ordinarily more useful for series connection, and the latter for parallel connection. The two symmetrised forms have the same resulting angle θ' but different surge impedances z_{0t} and z_{0p}, whose geometrical mean $\sqrt{z_{0t} \cdot z_{0p}}$ is the same as the geomean surge impedance z_{ab} of the original net.

8. In the series connection of identical dissymmetrical nets, the reverse-connection conditions are simple and the direct-connection conditions more complicated. In the parallel connection of identical dissymmetrical nets, however, the direct-connection conditions are simple and the reverse-connection conditions more complicated.

LIST OF SYMBOLS EMPLOYED

g Admittance of staff leak in a T or I (mhos $\angle$)
$g_1 g_2$ pillar leak admittances of a dissymmetrical Π or O (mhos $\angle$)
g' pillar leak admittances of a symmetrised Π (mhos $\angle$)
θ angle of a net (hyp. radians or hyps $\angle$)
$\theta' \theta'' \theta'''$ angles of identical nets (hyps $\angle$)
θ' angle of a symmetrised net (hyps $\angle$)
ϑ auxiliary hyperbolic angle used for computation of $\frac{q + q^{-1}}{2}$ (hyps $\angle$)
n n' integral numbers

ν architrave admittance of a Π (mhos $\angle$)
ν' architrave admittance of a symmetrised Π (mhos $\angle$)
q inequality factor of a net (numeric $\angle$)
$q'q''q'''$ inequality factors of identical nets (numerics $\angle$)
R staff impedance of a T or I (ohms $\angle$)
ρ architrave impedance of a Π (ohms $\angle$)
$\rho_1\rho_2$ arm impedances of a dissymmetrical T (ohms $\angle$)
y_0 surge admittance of a symmetrised net (mhos $\angle$)
y_{ab} surge admittance of a dissymmetrical net (mhos $\angle$)
z_0 surge impedance of a symmetrised net (ohms $\angle$)
z_{0p} surge impedance of a symmetrised Π (ohms $\angle$)
z_{0t} surge impedance of a symmetrised T (ohms $\angle$)
z_{ab} surge impedance of a dissymmetrical net (ohms $\angle$)
$z_{0a}z_{0b}$ terminal surge impedances of a dissymmetrical net at A and B ends (ohms $\angle$)

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RECENT PROGRESS IN THE FIELD OF OLD WORLD PREHISTORY

By GEORGE GRANT MACCURDY

(*Read April 26, 1930*)

A LITTLE more than a year ago, geologists and glaciologists were celebrating the one-hundredth anniversary of the birth of glaciology as a science. The year 1930 might well be chosen as the one-hundredth anniversary of the birth of prehistory as a science, since it was in 1830 that Thomsen of Copenhagen applied his new system of prehistoric chronology to the Danish National Museum collections. There is something back of the fact that these anniversaries come so nearly coinciding. Progress in glaciology has meant much toward progress in prehistory. Chronologically speaking, the Ice-Age falls within the limits of prehistoric time and the precision of our knowledge concerning glacial and interglacial chronology helps us to date many of the prehistoric relics inseparably linked with Ice Age deposits and fauna. In the past few years, much progress has been made in this field of research. In our summary then of recent progress in Old World prehistory let us first see what has been done in the way of correlating Ice-Age with prehistoric chronology.

Correlation of Ice-Age and Prehistoric Chronology.—For years the general consensus of opinion was that the last phase of Mousterian culture was coincident with the advance of the Würm or last glaciation and that Upper Paleolithic (Aurignacian, Solutrean, and Magdalenian) was coincident with a part of the maximum Würm glaciation and the major part of its retreat. Until very recently conservative prehistorians attempted to compress practically all of the Lower Paleolithic into the last interglacial epoch (Riss-Würm). In 1912, Commont had come to the conclusion that at least an early phase of the Chellean (Pre-Chellean) should be placed in the next to the last (Mindel-Riss) interglacial.

The recent progress in this direction is due largely to J. Reid Moir and the Abbé H. Breuil. The main points in Breuil's synchronism of European glaciations and European cultural epochs, slightly modified, may be tabulated as follows:

Glacial and Interglacial Stages	Cultural Stages		
Post-Würm			Tardenoisian Azilian Final Magdalenian
Würm II			Lower Magdalenian Solutrean
Laufen Retreat			Aurignacian Final Mousterian
Würm I	Levalloisian V		Mousterian
Riss-Würm Interglacial	Levalloisian III–V Micoquean		Early Mousterian Grimaldi phase Weimar phase
Riss	Derived and worn specimens of earlier cultures		
Mindel-Riss Interglacial	Upper Acheulian Middle Acheulian Lower Acheulian	Levalloisian II Levalloisian I Micoquean	Clactonian
Mindel	Derived and worn specimens of earlier cultures		
Gunz-Mindel Interglacial	Chellean Pre-Chellean	Early Micoquean	Base of Clactonian
Gunz Pre-Günz	Sub-Crag industry		Eolithic of some authors

Egypt.—A fairly complete story of the Stone Age in Egypt is slowly taking shape due to the researches of Miss G. Caton-Thompson and Miss E. W. Gardner and more recently to those undertaken by the Oriental Institute, University of Chicago, with K. S. Sandford and W. J. Arkell in charge.

The *Study of Nile-Faiyum Divide during Pliocene and Pleistocene Times* by Sandford and Arkell appeared in 1929 as Volume X of the Oriental Institute Publications.[1]

In Upper Egypt they have identified a series of four river terraces in which Lower and Middle Paleolithic implements occur in situ as follows:

Height above Nile	*Cultures*
3 meters (10 ft.)	Mousterian
9 meters (30 ft.)	Early Mousterian
15 meters (50 ft.)	Acheulian
30 meters (100 ft.)	Chellean

These have been traced over some hundreds of miles on both sides of the Nile and in adjoining deserts between Assuan and Assiut.

In Lower Egypt, between the Faiyum and Cairo, the series is not so complete. Thus far no representative of the 30-meter terrace has been discovered, but an old Nile channel at an elevation of 26–21 meters has yielded waterworn (derived) Chellean and fresh Acheulian implements. The Mousterian terrace in Lower Egypt has been traced through the Hawara Channel and into the Faiyum. The evidence shows that in Mousterian times the Faiyum was occupied by a vast lake fed by the Nile.

The Faiyum basin is an integral part of the Nile system; not a tributary system as once supposed, but an overflow reservoir into which the Nile discharged its surplus water. In Paleolithic times this Faiyum lake was full. The surface of the lake then stood at 34.2 m. (112 ft.) above sea-level. There was an annual flood and annual low Nile in Paleolithic times as there is today. Now the annual Nile flood reaches Cairo in late summer. Thus it may have been the early autumnal gales that swept from the west across the Faiyum lake, then at high level, which produced the storm beach some 10 feet (3.05 m.) high along the eastern shore of the lake. Strong westerly autumnal gales still sweep across the Faiyum basin; but on the shores of the shrunken survivor of the old

[1] Vol. I of Prehistoric Survey of Egypt and Western Asia.

lake now hidden far below sea-level, they produce a storm beach barely a foot high (30 cm.).

Along the eastern shore of the old Faiyum lake a short distance south of the Philadelphia ruins, there are important implement-bearing deposits. The site is at Gebel er-Rus, the implements are of Mousterian age and the elevation is 34 m. (115.5 ft.) above sea-level. The richest site thus far discovered in the Faiyum is on the beach at Ḳasr Basil near Tutun on the southern shore of the old lake and at an elevation of 34.2 m. (112 ft.). There is a basal pebble bed with Mousterian implements on which is superposed a silt deposit in turn covered by gravel, both containing Mousterian implements. The cores or nuclei as well as the finished tools resemble those found in Mousterian stations of western Europe. The cores vary from elongated to squarish ("double-ended") and discoid forms. The material employed was of flint or chert.

In the kitchen middens and the silts associated with them in the Kom Ombo plain, between Edfu and Assuan, E. Vignard has found an industry which seems to have evolved locally from the Mousterian, but which in time ceased to be recognizable as Mousterian, owing to an accumulation of modifications and the growth of new types. To the modified indigenous industry Vignard has given the name Sebilian. He divides it into three phases: Sebilian I, II, and III. The Mousterian disk persists in Sebilian I. The microlithic flakes and implements of geometric design in Sebilian III are comparable in technique with the Tardenoisian industry and may be of the same age.

Sandford and Arkell find that the Sebilian industry described by Vignard is not confined to the Kom Ombo plain. They find it on both sides of the Hawara Channel, especially at Kom Medinet Ghurab and Dimishḳin, in silt and fine gravel. Within the Faiyum, the gravel banks flanking the Hawara Channel spread out into a second system of beaches, storm beaches and shoals closely resembling those associated with the lake of Mousterian times. The water in the lake

during the Late Paleolithic or Sebilian times stood at 28 m. (92 ft.) above sea-level, or 6.1 m. (20 ft.) below its elevation in Mousterian times. An exceptionally prolific Sebilian site was found on the shore of this Late Paleolithic lake near Philadelphia. The beach of this old lake is a prominent feature in the great bay of Philadelphia and the greater part of the Greco-Roman town was built upon and just behind it. In an obscure corner of the Faiyum basin between Shaḳluf Bridge and Ḳasr Basil, Sandford and Arkell were so fortunate as to find no less than ten beaches left by the falling lake as the climate became progressively more arid.

The industry of Late Paleolithic or Sebilian age found at the 28 m. (92 ft.) beach level is of surprising uniformity. The cores are miniatures of the Mousterian cores. The flakes fall into two classes: (*a*) miniatures of the Mousterian flake and (*b*) thin broad flakes with straight parallel margins. Most of the microlithic Sebilian types of Upper Egypt still remain to be discovered in Lower Egypt.

The field work of Sandford and Arkell terminated with the 22.5 m. (74 ft.) level. However they agree with Miss G. Caton-Thompson and Miss E. W. Gardner that the 22.5 m. (74 ft.) lake was succeeded by a Neolithic lake at a level of 17.4 m. (57 ft.); that these two successive lakes were separated by a long interval of time, during which the Faiyum basin was drained and the old lake deposits were deeply eroded before the water rose again to the 17.4 m. (57 ft.) level, decadent Neolithic industries being associated with it down to 2.1 m. (7 ft.) below sea-level; that it has since continued to sink until the present day, when its surface lies 44.8 m. (147 ft.) below sea-level and only 5.5 m. (18 ft.) above the bottom of the deepest part; that the last stage of contraction has been accompanied by a sudden increase in salinity, which has killed most of the fresh-water fauna, two marine bivalves taking its place. Desert conditions were apparently established in Upper Egypt as early as Sebilian times, and at a later date as one proceeds northward, north of the latitude of the Faiyum, they may not have become absolute until post-Neolithic times.

Mesopotamia.—There is every reason to believe that records similar to those reported from the Upper Nile valley will be found in the upper stretches of the Euphrates and Tigris valleys. In fact, Passemard has reported the finding of a Chellean hand ax from the gravels at the base of the 30 meter terrace, right bank of the Euphrates between Rakka and Deir ez Zor, Syria. In type and patina it is exactly like those found in the Thames valley at Milton street and in the Somme valley. Passemard also points out that in the Euphrates valley one finds a system of terraces at four levels—15, 30, 60, and 100 meters respectively—comparable with the four terraces to be found in the river valleys of western Europe.

France.—Even older than the Chellean of the 30 meter terraces in the valleys of the Nile and the Euphrates are the crude artifacts found by Peyrony only last summer in the basal deposits of the station known as La Micoque, near Les Eyzies (Dordogne). The site has been known for at least 35 years, beginning with the excavations of Chauvet and Rivière. Many prehistorians have since done at least some work on this spot. All were agreed as to the relatively great antiquity of the deposits, dating back at least to the Acheulian Epoch. Each stopped at a thick sterile deposit supposed to underlie the oldest relic-bearing level. It remained for Peyrony to hazard a sounding deeper than all the others. He was rewarded by finding a basal relic-bearing deposit far below the lowest hitherto known. The artifacts are eolithic in type and are referred by Peyrony to the Pre-Chellean Epoch (Lower Pleistocene). Peyrony has also just added another page to our knowledge of another site in the Dordogne—the type station of Le Moustier. The oldest relic-bearing level hitherto known at Le Moustier belongs to the Mousterian Epoch. Beneath this is a thick sterile deposit beneath which Peyrony has found an additional relic-bearing deposit. But the cultural remains are of a type similar to those in the level above. Peyrony's successful soundings at La Micoque and Le Moustier should serve as a reminder to all excavators to be

sure they are at the bottom of a section as soon as possible after their work is begun.

By combining four rock shelters in the Dordogne—La Micoque, Le Moustier, Laugerie-Haute, and Laugerie-Basse, —it is now possible to build up a composite section representing the entire gamut of Stone Age cultures from the Pre-Chellean to the Neolithic, inclusive.

Chou Kou Tien.—The year 1929 will compare favorably with any single year, in the past one hundred, in respect to prehistoric achievement. The discovery of an almost complete skull of an early type of man at Chou Kou Tien southwest of Peiping (Peking), China, is perhaps the outstanding event of the year. The first discovery of hominid remains at Chou Kou Tien was some four years ago.

In 1926 Zdansky recovered two human teeth from material that had been previously taken from the Chou Kou Tien deposit: a right upper molar and a lower first premolar. In October of the same year Dr. Bohlin found a left lower permanent molar (probably the first) of a child.

During the season of 1928 there were found in the same deposit (Locus A) the right horizontal ramus of an adult human lower jaw with three molar teeth in situ and leaving the premolar, canine, and distal half of the lateral incisor sockets preserved; a somewhat worn right upper molar (M 1 or M 2) showing definite evidence of injury during life; the labial side of the crown and portion of the root of a permanent upper median incisor and immature lower (?) permanent incisor; and lastly the labial half of the crown and root of a worn lower median permanent incisor, posthumously crushed and deformed. The specimens are deeply pigmented and mineralized in a manner characteristic of all fossils recovered from Locus A.

The fossils from Locus B. of the same Chou Kou Tien deposit are not deeply pigmented, most of them being quite white or of light buff color. They are imbedded in a hard yellowish travertine. Not much of the hominid material has as yet been disengaged from the travertine matrix, enough

however to prove that the hominid material from both loci belong to the same genus and species, to which Dr. Davidson Black has given the name *Sinanthropus pekinensis.* The specimens thus far disengaged include a score or more of both deciduous and permanent teeth representing many phases of wear and age, together with complete symphysis region of the lower jaw of a very young individual; other specimens are already visible in the matrix.

In December, 1929, while excavating a sheltered recess of the main deposit at Chou Kou Tien, Mr. W. C. Pei discovered the greater part of an adult hominid cranium in a good state of preservation (not even crushed). Within the main cave deposit at Chou Kou Tien up to the present time *Sinanthropus* remains have been recovered from five different loci, three of which, including the last major find, have been discovered by Mr. Pei during the last season's work. Contrary to any reports which have been circulated, no skeletal parts other than the skull and numerous isolated teeth have been recovered during the past year's excavations.

It should be noted that the different *Sinanthropus* loci discovered within the main Chou Kou Tien deposit are all clearly contemporaneous with one another, being Lower Quaternary (Polycene) in age. This latter statement is based on the evidence collected in a preliminary report on the geology and palæontology of the site by Père Teilhard de Chardin and Dr. C. C. Young, which is now in press. Further it should be added that up to the present time, though thousands of cubic meters of material have been examined, no artifacts of any nature have been encountered nor has any trace of the usage of fire been observed.

The greater part of the left lateral surface and the fore part of the base of this unique skull of *Sinanthropus* is still imbedded in a block of very hard travertine. The vault of the skull, from its massive brow ridges to the occiput, and the whole right side of the specimen was however supported within a relatively soft matrix which has now been removed. In the present stage of its preparation it thus becomes

apparent that the brain case has been almost completely preserved while most of the facial region would seem to be lacking. Black says the cranium is that of a young adult female.

The skull of *Sinanthropus* would seem to be of approximately similar length to that of *Pithecanthropus* and like the latter form is provided with massive brow ridges, a feature to be correlated with a powerful jaw mechanism. However, *Sinanthropus* characteristically differs from the Java type in the following important features: relatively well developed frontal eminences, well localized parietal eminences and greater height of skull vault, all these characters pointing to a relatively greater brain capacity in *Sinanthropus*. The mastoid processes of *Sinanthropus* are small. The sockets in which the lower jaw articulated are well preserved on both sides, a circumstance which will be of great value in the restoration of the lower jaw fragments recovered in 1928.

Saccopastore.—The estate of Saccopastore is about 3.5 km. (2.2 mi.) from the Porta Pia, Rome. Here in a gravel pit workmen discovered a skull of the Neandertal type in 1929. It was immediately turned over by the lessee of the estate, Signor Casorri, to the Duke of Grazioli, who confided it to the Anthropological Institute of the University of Rome, where it will be preserved. According to Professor Sergio Sergi, the mandible is lacking but the cranium is in a fairly good state of preservation.

The cranium is characterized by the relatively large facial in contrast with the cerebral portion, as well as the high degree of prognathism and the depression of the vault. The cranial capacity is not over 1200 cc. and the cranium is that of a female. The remaining teeth, five molars and a premolar, are large. The orbital apertures are very large; the piriform aperture is low and large. The anterior projection of the nasal processes and of the body surface of the maxilla form a kind of snout, not met with in any other known type.

The cranium came from a depth of 6 m. (19.6 ft.) in a stratum of sand and gravel, rich in fossil animal remains:

Elephas antiquus, *Hippopotamus major*, *Rhinoceros mercki*, *Cervus elaphus*, *Bos primigenius* and others. No artifacts have been found at Saccopastore, but artifacts of the Mousterian type were found in the same strata of nearby sites in the valley of the Tevere and Aniene as early as 1846. These finds with that of the cranium are proof that man lived in Lazio probably as early as the Riss-Würm interglacial Epoch.

Czechoslovakia.—There are many sand pits and brickyards within and near the city limits of Brno. These are watched continuously for prehistoric relics. In May, 1927, a human skeleton was found in the exploitation in Sušilova street, Zabovresky quarter of the greater city. The skeleton was found in a red sandy loam at the contact between the sand and the coarse gravel at a depth of 2.15 m. (7 ft.). It lay in a crouching position in a kind of egg-shaped hollow. The color of the sandy loam about it was deep red. Since the color diminished in intensity radially in all directions from the skeleton, the coloring matter must have been originally a part of the burial. The skeleton, which was in a bad state of preservation, is that of an adult woman and is referred to as Brno III. According to Matiegka, the skull bears the strongest resemblance to the skulls from Combe-Capelle and Mladeč I; also in certain features to Brno II and the female skulls from Předmost.

An additional fossil human skeleton was found at Předmost in 1928. The skeleton, in a fragmentary condition, was found in ashes of the Paleolithic layer, associated with bones of the mammoth. The proximal end of the left human femur is missing. On the remaining two-thirds, there are scrapings and cuts oblique to the axis of the shaft on its anterior surface all the way up from the lateral condyle. They were done by flint tools and raise the question as to whether cannibalism was practised at Předmost. In all, 54 bones of the human skeleton were found, at the same level and not far removed from the remarkable communal grave discovered by Maška.

Our knowledge of the fossil fauna associated with fossil man is growing apace. One of these animals was the woolly

rhinoceros (*Rhinoceros tichorhinus*) which sometimes served as a model for Cro-Magnon artists. It was a contemporary of the woolly elephant (*Elephas primigenius*). Well preserved examples of both have been found in Siberian ice fields. In 1907 asphaltum deposits at Starunia, Poland, yielded a mammoth (woolly elephant) and woolly rhinoceros, both perfectly preserved. They are now in the museum at Lemberg. In 1929, from the same deposit there was taken another complete woolly rhinoceros, which has been removed to the museum of the Polish Academy of Science in Krakau.

Cave Art.—The latest researches of Pittard tend to prove that the first example of cave art to be discovered was not the engraving on bone from Chaffaud, but an engraved baton of reindeer horn from the cavern of Le Veyrier, near Geneva (Haute-Savoie). This baton was found by François Mayor in 1833 and reported the same year to the *Société de Physique et d'Histoire Naturelle*, Geneva. The engraving is not an important work of art. It would be difficult to determine the animal intended to be represented. Mayor also found that which at first glance would seem to be a harpoon. But the point is at the wrong end; so that instead of being a harpoon shaft with barbs, the piece represents a stem in bud. Thus the first engraved object and the first sculptured object dating from the cave-art era were both found at Le Veyrier—on French soil, to be sure, but by a Genevese.

A number of outstanding discoveries in the field of Paleolithic art have been made during the past few years, some of which have not yet been published. I shall mention briefly six of these—one each from Siberia, Italy, England, and Germany and two from France.

Irkutsk.—A human female figurine has been found on the Bjelala river near Irkutsk, Siberia. The site is not far from the Chinese frontier. In the same deposits were found fossil bones of the mammoth and woolly rhinoceros. The figurine is of the Brassempouy-Willendorf type. The distance from Brassempouy near the Bay of Biscay to Irkutsk is about 11,200 km. (7,000 mi.).

Savignano.—A human female figurine was encountered when digging a cellar in 1926 at Savignano-sul-Panaro (Emilia), Italy. It is of the Brassempouy-Grimaldi type and made of steatite. The head is not differentiated, the feet and arms are not represented, the legs are fused, while the breasts, hips and ceinture are over-emphasized. The figurine is 22.5 cm. in length.

Petersfels.—Petersfels is the ninth prehistoric station in Germany, where Paleolithic art has been found. It is named for Herr Peters, the discoverer. The site is near Engen (Hegau), southern Baden, only a short distance north of Kesslerloch in Switzerland, which many years ago yielded the well-known figure of the browsing reindeer. The find includes stone artifacts, bone needles, perforated shells and teeth, javelin points, batons of reindeer horn, decorated ivory disk, pendants representing the human female form carved from lignite; also engravings on bone and reindeer horn, including figures of the reindeer on a baton. The female figurines are perforated for suspension and are of the Brassempouy-Willendorf type; the head is no longer recognizable, the legs barely indicated and the hips exaggerated. Petersfels belongs to a single phase of the Lower Magdalenian Epoch.

Trois-Frères.—In addition to the long series of mural figures discovered at Trois-Frères, recent excavations have yielded remarkable examples of portable art. These finds were made in floor deposits not far from the Enlène entrance, by Mons. Louis Begouen. They include a plaque of ivory on which are engraved figures of two cats seen from the back, also a cricket in profile. Another example is a baton of reindeer horn with engraved figure of a bison. Perhaps the rarest of all is the head of a wild goat carved from reindeer horn with inlay eyes of another material (burnt bone).

In the summer of 1929 Mons. Begouen found still another example of Paleolithic inlay. It is on the base of a dart thrower, of which the crochet is formed by a bird's beak. On one face of the shaft of the dart thrower there is engraved a goose, the neck bent; on the opposite face the same bird is

sculptured in high relief, the head turned backward. The eye is represented by a deep pit in which there was originally an inlay.

Le Roc.—The station of Le Roc is situated on a small affluent of the Echelle, in the commune of Sers, some 15 km. southeast of Angoulême (Charente). It consists of two caves—grotte du Roc and grotte de la Vierge—between which is a workshop where the sculptured frieze was found. In addition to the sculptured frieze, engraved figures on limestone were found as follows: bison and cave bear (on the same piece) from below the grotte de la Vierge and a horse from the same trench; a third example is the figure of a bison.

The frieze, which is now installed in the *salle Henri Martin* at Saint-Germain-en-Laye, is sculptured on a series of five stones. When found these stones were turned with their ornamented faces against the archeological deposit. As reconstructed, the series from left to right is as follows: first stone, at left, masked man (?), center two horses, right musk ox charging a hunter; second stone, horse facing left; third stone, horse facing right; fourth stone (beneath the third), horse facing right; fifth stone, horse and pseudo-bovidæ both facing right.

El Pendo.—The first example of cave art ever discovered was an object of the type later known as *bâton de commandement.* The baton is a piece of reindeer horn or stag horn, with one or more perforations and usually decorated. Paleolithic batons have been found sparingly in Austria, Belgium, Czechoslovakia, England, Germany, Italy, Poland, Siberia, and Switzerland. Several dozen have been reported from France, and nine from Spain. The latest one from Spain was unearthed in the cave of El Pendo (Santander) by the Abbé Jesus Carballo and is preserved in the *Museo Prehistorico,* Santander. From the viewpoint of art and symbolism, this specimen ranks among the most important. It is of stag horn with a perforation near one end. The entire superficies is practically covered by a series of admirably incised animal heads: four of *Cervus elaphus* (one stag and three hinds) and

one of the horse. These are accompanied by groups of incised lines, some in parallel and some X-shaped. This baton furnishes added evidence that the cave artist showed a preference for the female of the species. The three heads of the hind exhibit the same technique as does the hind's head on the baton from Valle (Santander). The baton from El Pendo was found in the Upper Magdalenian level.

Creswell Crags.—From England, poor in Paleolithic art, Leslie Armstrong reports the finding in a proto-Solutrean horizon of one of the caves at Creswell Crags (Derbyshire) the figure of a man wearing an animal mask, engraved on reindeer bone.

Jordansmühl.—I cannot close this report of progress without mention of at least a few recent discoveries of post-Paleolithic date. There have been many, out of which Jordansmühl (Silesia) should be first considered. The name is well known in prehistory through discoveries made at the Neolithic and Chalcolithic camp site on Bischkowitz hill. In the vicinity of Jordansmühl (west side) are a number of sites dating from the Bronze and Iron Ages, as well as from the Neolithic.

The Jordansmühl ceramic type is distinct from all others. The paste is free from coarse material, the walls of the vessels are thick. The color is dark brown to iron grey. Other types than the Jordansmühl are found in the vicinity.

In 1925 a clay figure of a ram was found in a sand pit on the Klose farm west of Jordansmühl. With it were two clay vessels and a broken flint knife. Although somewhat weathered the figure of the ram has been restored. Its total height is 33 cm., body height 23.5 cm. and length 37 cm. The rugosity of the massive horns is well represented by means of transverse string imprints. The neck and body also bear string imprints. This is a splendid example in clay of the Neolithic potter's art.

Neolithic Art in China.—The East India Museum of Stockholm has recently come into possession of a ceramic piece dating from the Neolithic Period in China, which is of

more than passing interest. It is the lid of a vessel in the form of a human head, neck and bust. The facial region rises slightly from the spherical head. The chin, nose and cheek protuberances are perforated; the eyes and mouth are simply oval holes. The brows are prominent. Above and back of the brows there is a pair of pitted protuberances. Beginning at the crown and meandering gracefully down the back of the head and neck onto the bust there is a figure suggesting a Chinese pigtail, but instead it is a serpent, the head (with open mouth) of which rises above the pair of protuberances. Nothing like this has been found in western Asia or south-eastern Europe.

Iberian Ceramic Art.—An idea of the excellence of the potter's art in the Iberian Peninsula some 400 B.C. may be had from a study of the Heiss collection, recently published in part by Hugo Obermaier and Carl Walter Heiss. The necropolis from which this collection came is in the region of Archena (Prov. Murcia), southeastern Spain. The paste is a fine reddish yellow clay, the ornament in two colors. A description of one of the vases will suffice to give a fair idea of the group as a whole. The horizontal bands dividing the vase into zones are reddish brown, whereas the zonal decorations are in red. The principal panel (at the shoulder) carries on one side a bird with outstretched wings, on the other a carnivore with open mouth revealing teeth and tongue. Beneath the bird on one side and the carnivore on the other there are in each case a lone elevated bird's wing and a fish. The rest of the ground in each panel is appropriately decorated with plant-like motives. Of the other narrower zones, the middle one is filled by means of a series of S-shaped figures, while each of the two zones below carries a series of concentric half circles. The vase has a pair of vertically placed handles on the shoulder separating the two panels of the principal zone. The ceramic art of Archena is in all respects similar to that in the neighboring region of Elche.

Iraq and Palestine.—In closing this account of progress, it is fitting that mention be made of the work being carried on by

the American School of Prehistoric Research, especially that in coöperation with the British in Iraq and Palestine. The first joint expedition was in that part of the hill-country of Southern Kurdistan now included in the kingdom of Iraq. This region is called Sulaimani and the town of Sulaimani, which lies 265 km. northeast of Baghdad and 45 km. west of the Persian border, has sometimes been called the capital of Southern Kurdistan. Nearly eight weeks in the autumn of 1928 were devoted to reconnaissance and digging.

The cave of Zarzi, 50 km. northwest of Sulaimani, was completely excavated. The chief culture-bearing level at Zarzi is Upper Paleolithic, the equivalent of the Upper Aurignacian at such stations in Europe as Willendorf and Krems in Lower Austria and the caves of Grimaldi in Italy. At the top of this level, there were found true Tardenoisian types of the Mesolithic Period. The layer directly above this was mixed and yielded crude pottery as well as flint implements similar to those found below.

Work was continued at the caves of Hazar Merd, about 8 km. southwest of Sulaimani. One of these—the Dark Cave (Ashkot-i-Tarik)—was partially excavated. Here three levels were encountered in the floor deposits, the sequence from above downwards being:

A. Bronze Age to recent.
B. Upper Paleolithic (similar to that at Zarzi).
C. Mousterian.

Miss Dorothy A. E. Garrod, representing the Percy Sladen Memorial Fund, was in charge of the expedition. Her report [1] has just been published in Bulletin Number 6 of the American School of Prehistoric Research (New Haven, Connecticut, March, 1930).

For three months during the spring of 1929, the American School of Prehistoric Research and the British School of Archæology at Jerusalem made soundings in three caves at Jebba near Athlit, Palestine—Mugharet el Wad (Cave of the

[1] The Palæolithic of Southern Kurdistan: excavations in the caves of Zarzi and Hazar Merd.

Valley), Oven Cave, and Cave of the Kid—and partially excavated one, the Mugharet el Wad. The locality is 16 km. south of Haifa and less than 5 km. inland from the Mediterranean shore.

Mugharet el Wad proved to be a very rich site, containing also a more complete series of culture levels than any other site thus far discovered in Palestine. Beginning with the Mousterian Epoch, the cave was occupied almost continuously down to the present. The sequence from the top downwards is as follows:

7. Mixed layer with remains of Arab, Byzantine, Roman, Greek, late Bronze and early Bronze periods.
6. Mesolithic.
5. Upper Paleolithic—Capsian in type, showing probable relation to African culture.
4. Middle Aurignacian in type.
3. Middle Aurignacian in type.
2. Early Middle Aurignacian in type.
1. Mousterian in type.

The artifacts of the Mesolithic layer included small flint crescents and sickle blades, the latter polished through usage along the toothed margins; cores of various sizes and shapes, knives, scrapers, awls, etc. There were bone points, many containing one end intact to serve as a handle; some were small enough to be needles but without an eye, other small ones were pointed at both ends and evidently served as fish hooks; bone harpoons, bone polishing tools, and a bone grooved for hafting microliths or perhaps sickle blades. Objects of adornment included small bone pendants, a bone bead, and various teeth cut and pierced for stringing, the canine teeth of a small carnivore being especially numerous.

The most remarkable of all the finds from this level was a small human head carved from a black and grey banded pebble. The hair and back of the head were left unfinished. The face was done with care: long thick eyebrows, large long eyes, well executed broad but not very flat nose, and prominent lips. The neck was partially completed. The whole is about

as large as the end of a man's thumb; this is the oldest human representation found in Palestine to date and may correspond in age to the late Magdalenian of Europe.

The Mesolithic layer of Mugharet el Wad has thus yielded objects hitherto undiscovered in Palestine: the stone human head, the bone haft for microliths and the bone harpoons. To these should be added from the same site: a pierced bone resembling a baton and an animal figure carved in the end of a fragment of long bone, both found previously by Mr. Lambert, assistant Director of the Department of Antiquities of Palestine.

Another find new to Palestine was the multiple human burial found by the joint expedition in the Mesolithic layer. This consisted of ten skeletons—four adults and six children including infants. These had been piled on a hearth prepared for the purpose. The burials were all extended and at various angles. The armbones of two were interlocked. The head of an infant had been placed on the shoulder of one of the adults. A deer antler, some of the bone pendants and the stone head were all from this burial place, the stone head being directly beneath the skeletons. An additional skeleton was found at the same level in a recess and lying upon its right femur was yet another skull. A flexed skeleton was found in the terrace outside, not far from where Lambert had previously found two skulls only.

Below the Mesolithic, there are four occupation levels which can be definitely referred to the Middle and Upper Aurignacian. In this Upper Paleolithic deposit were found two human jaw fragments and other fragments of human bones, also a number of large human teeth. These and the Mesolithic skeletons will be described by Sir Arthur Keith. The Aurignacian is also rich in artifacts. Beneath the Aurignacian deposit, there is a Mousterian layer some two meters thick. The second joint Palestine expedition is now (1930) completing the excavation of this deposit; it is also excavating the nearby Oven Cave and Cave of the Kid.

GLOZEL: A MYSTERY[1]

By DAVID RIESMAN

(*Read April 24, 1930*)

THOSE who have read something about Glozel may wonder why I speak of it here. Is it not a dead issue? No, for as a study of human credulity and as a commentary on the hot-headedness or should I say pig-headedness of many men of science, it will always occupy a prominent place in the history of civilization.

Although many of the audience are probably familiar with the main facts, I want to give a brief synopsis of the involved story. I say "story" advisedly for from the very outset the mystery of Glozel has formed a fascinating tale; very much in the genre of our best thrillers, with plot and counter-plot, gum shoe detectives and all the pertinent paraphernalia.

Glozel is a small hamlet of four farmhouses, about fifteen miles from the famous spa of Vichy. Emile Fradin, then a youth of eighteen and belonging to an old local family, was one day working in his grandfather's field when a cow suddenly slipped into an unsuspected hole. Fradin went to investigate and found that the hole led into an oval pit containing a variety of remarkable objects—bricks, tablets, vases, which he gathered and as soon as possible showed to the village school mistress Mlle. Picandet. The latter in turn showed some of the tablets to M. Clément, a school teacher in la Guillermie. Eventually the news of the discoveries came to the ears of Dr. Albert Morlet, a surgeon of Vichy and an amateur archeologist. Thereafter Dr. Morlet and Emile Fradin together began to excavate at Glozel and brought to light more and more buried objects which they collected in grandfather Fradin's house and which Dr. Morlet described in detail in an endless series of articles in a literary journal, the *Mercure*

[1] With lantern slide demonstration.

de France. It was through this magazine—the Atlantic Monthly of France in more senses than one—that I became interested in the Glozelian discoveries. My interest was especially aroused by the claim of Morlet and others that an alphabet had been discovered at Glozel which antedated every other alphabet then known. I therefore decided while spending a vacation in the Auvergne to see Glozel for myself, but before doing so I determined to interview Dr. Morlet in Vichy. At first he suspected me of being an archeologist but when in answer to a direct question, I denied the soft impeachment and proclaimed myself merely a doctor, he became cordiality itself and showed me his collection of Gallo-Roman and Glozel antiquities. He told me that he as well as others had been inclined to consider Glozel as belonging to the Magdalenian age because of the presence of harpoons and of stones engraved with reindeer and other animals long extinct in France but further studies had led to the conclusion that Glozel was Neolithic. Dr. Morlet kindly asked me to stay over until the following day and dig with him and Professor Björn of Sweden but I was unable to do so.

After leaving Morlet I motored, together with two American friends, to Glozel. Emile Fradin received us and at once offered to take us to the field of excavation. It was at the bottom of a deep ravine and was surrounded by a barbed wire fence. He showed us the original oval pit and the two tombs subsequently discovered. As it was raining hard and as the clayey ground was slippery, I declined his invitation to crawl into one of the tombs but asked instead to see the Museum. After paying two francs each we entered through a low door above which was a crude sign with the pretentious words, Musée de Glozel, and found ourselves in a square low-ceilinged room with shelves on the walls and very primitive glass cases standing on the floor. The objects exhibited on shelves and in cases were astounding in number and variety—vases, tablets, engraved stones, ornaments especially pendants, some pieces of glass, and harpoons, the last not nearly as artistic as those of Magdalenian age I had seen at Les Eyzies

and at Laugerie-Basse. Three articles attracted my special attention—vases or vase-like pottery-ware having eyes, nose and ears but no mouth which Morlet has called death masks, explaining the absence of the mouth by assuming that the primitive makers wanted to express the silence of the grave. Secondly, a squarish object suggesting the female figure with a cylindrical projection from the forehead interpreted as the phallus—this Fradin told me was a bisexual idol; and most striking of all clay tablets with graven signs looking in every way like alphabetical characters. I was struck by the clean red color of these tablets. When I spoke of this to Fradin, he explained it by saying that the soil in which the tablets had been found was such that it did not readily fuse with the clay and hence was easily brushed off. There were also some large casts of the human hand which differed from the imprints of the hands in the Spanish and French caves in having all the fingers present.

I offered to buy some of the articles, especially a tablet, but Fradin resolutely refused to sell. During the whole of our stay in the Museum, the grandfather stood silent and motionless in a doorway leading to an inner room.

The reputed discoveries of an alphabet dating back to Neolithic times of which I had now seen the alleged evidence in abundance created a tremendous sensation in informed circles. Altogether about 136 characters have been distinguished, representing every letter of the alphabet except the letter B.

Hitherto the credit for creating an alphabet had been given to the Phœnicians but the oldest known Phœnician record found at Byblos a few years previously dated back only to about 1300 B.C.; Sir Arthur Evans' baffling Cretan inscriptions to ca. 3000 B.C. If Morlet and those who agreed with him were right, then Glozel was truly what M. Reinach called it—one of the greatest archeological discoveries of all time.[1]

Almost immediately after the first appearance of Morlet's reports, doubts began to be voiced about the authenticity of

[1] Dr. Hackh in an exhaustive essay on the History of the Alphabet makes no reference to Glozel—if true, Glozel belongs at the very bottom of the linguistic tree.

Glozel, although in the early period of the controversy there were perhaps as many scientists who accepted the discoveries in good faith as there were doubters. Soon the pro- and anti-Glozelians became personal and attacked each other with a vituperative vehemence and a destructive sarcasm for which the French language appears to be the ideal medium. Reputations were shattered, old friendships broken, and as one of the French dailies remarked—even butcher boys came to blows on the streets of Paris. In fact Mrs. O'Leary's cow did no greater damage to Chicago than Fradin's to the reputation of many French savants.

Before long the leading French prehistorians with only an occasional exception began to deny the authenticity of Glozel and to declare the excavated articles to be forgeries. A number of Englishmen were likewise unconvinced. However, Dr. Foat, a London scientist, makes the categorical statement that "if the finds of Glozel are not authentic, it is equally necessary to consider as false all that I have seen in museums between London and Constantinople." Several Scandinavian, Belgian and Portuguese scientists also supported Dr. Morlet and a German, Dr. Wilke, in a recent article enthusiastically upholds the standard of Glozel.

Are Dr. Morlet's supporters right or is Glozel but one more of the long series of frauds that history recounts since Jacob imposed upon his father Isaac? Many will come to your minds—Thomas Chatterton, our own Dr. Cook, Constantine Simonides, the pretended author of the Codex Sinaiticus, the forger of the Mecklenburg Declaration, the Lincoln love letters in the *Atlantic Monthly*, the Tiara of Saïtapharnes, and countless others. Two perhaps are germane and worth recounting. In the early eighteenth century George Psalmanazar, born in the south of France, came to England and with the connivance of a rascally clergyman, Alexander Innes, proclaimed himself a native of the Island of Formosa. He was lionized in London, wrote a description of the Island which he had never seen and included in the book an alphabet and grammar of the Formosan language. Though many

doubted his veracity, the book passed through two editions and was translated into French. Toward the end of his life he revealed himself in his own memoirs as a colossal faker and declared that all he had published including the language and the grammar was a hoax.

One of the most interesting cases and the one having the greatest analogy to Glozel, if Glozel be a fake, is that of the so-called "Figured Stones of Würzburg."

In the first half of the eighteenth century there lived in Würzburg, in Bavaria, an ultra-pious physician named Johann Bartholomaeus Adam Beringer. He is not remembered for any great discovery or contribution to science, but for his share in a remarkable scientific hoax. At the time in which Beringer flourished an active discussion was going on as to the source and meaning of fossils. Although Leonardo da Vinci had understood their true nature—even Herodotus, 400 B.C. had a correct idea—the scientists of two hundred years ago accounted for them as the result of "stone-making forces" of "formative qualities" or as growths from seeds. We may be inclined to smile, but with Dayton in Tennessee to chasten us, we cannot throw stones at the Würzburg of two centuries ago nor at the Sorbonne which a hundred years later deprived the great Buffon of his chair because of his heterodox theories.

Beringer had committed himself publicly to the belief that fossils were the capricious fabrication of God, hidden by Him in the earth for some inscrutable purpose. His zealous maintenance of this fundamentalist position led some of the students together with members of the faculty and wags of the town to make numerous fossils of clay which they buried in the side of a hill where they knew the Professor was wont to search for specimens. Beringer chancing upon these objects was completely deceived. The jokers became bolder and buried the most extraordinary and extravagant figures their whimsical imagination could suggest. They fashioned tablets bearing inscriptions in Hebrew, Babylonian, Syriac and Arabic and buried them not far from the original spot. Beringer was

overjoyed to find such abundant confirmation of his doctrines and forthwith in true German fashion proceeded to write an exhaustive treatise. The wags now began to realize that they had gone too far. They expostulated with him and even told him the whole truth. Instead of believing them Beringer became more than ever convinced that the story his frightened colleagues told him was a ruse to rob him of the honor of his discoveries. No one could stop him. At great expense he published in 1728 the "Lithographiæ Wirceburgenses."

Only too soon the shout of laughter with which the book was greeted brought the truth home to him. In chagrin and despair he exhausted nearly his entire fortune in a fruitless endeavor to suppress the edition and to buy up the copies already issued. He died soon afterwards, it is said, of a broken heart.

Is Dr. Morlet like Johann Beringer the victim of deception? Upon me personally he made the impression of an honest man. In certain quarters he was accused of fraud, for example, by the *Journal des Debats* and by the French Society of Prehistory. He promptly brought suit against them and won a verdict of 1000 francs damages. The defendants carried the case to the Court of Appeals at Riom, the native town of Willa Cather's lovable archbishop. In confirming the verdict, the Court gave expression to an amusing quibble. It held that Morlet being a surgeon by profession and only by avocation a pre-historian, was not injured in the eyes of his real colleagues but only as an amateur archeologist. However, as the defendants had not actually proved fraud, they were declared guilty, but the fine was reduced to one franc and costs.

Another humorous episode might be mentioned. Regnault, President of the French Society of Prehistory, sued a M. X because he, Regnault, had been compelled to pay the sum of 4 francs to see a collection of fake objects. As part of this legal action, the police of Moulins broke into Fradin's premises and took away a number of objects which were afterwards submitted to the public expert, M. Bayle. The

latter reported that the tablets were of recent manufacture. Pieces of clay from a tablet crumbled readily in water; hence it was not conceivable that the tablets could have resisted the moisture in the ground had they been there for many years. Furthermore, a bit of grass picked out of a piece of earthenware showed under the microscope the vegetable cells, and some of the bone instruments still contained marrow. Bayle was soon afterward shot to death by one Philopponet against whom he had testified in court.

The Fradins themselves brought suit against M. Dussaud, member of the Institut, who in a trenchant brochure had called them fakers.

Let us now delve a little more deeply into this mystery so that we may understand better the basis of the controversy. The first serious doubt as to the authenticity of Glozel was based on the heterogeneity of the articles in Fradin's museum. How could one explain the presence of so many dissimilar and unrelated objects—the two or three thousand at the time of my visit have now grown to five thousand—in one small field of excavation. No other archeologic site offers a parallel. Morlet answered this by saying that Glozel was a Champ des Morts, a cemetery, and that as among many primitive peoples of later times, everything belonging to the dead had been buried with him. C. Jullian who considers Glozel a Gallo-Roman station accounts for the multiplicity of objects on the assumption that Glozel was a sorcerer's sanctuary. He has added greatly to the gaiety of nations by attempting a full translation of the inscribed tablets from the published illustrations. Dr. Morlet showed me with much amusement a crack in one of the tablets which Jullian had translated as a character.

Aside from the puzzling complexity of the collection, it has been pointed out that the tablets first exhumed bore fewer and less perfect characters than the later ones. Further, as soon as some one had made a criticism, the objects next exhumed were free from the criticized defect. Quite often certain features appeared that could be traced directly to

scientific articles published shortly before. These facts seem of course very significant. Moreover, the scratches on stones whether representing animal figures or alphabetic characters were without the patina covering other parts of the stones; this would suggest recent production. Much was made of the penetration of roots into vases or tablets; but upon examination these roots were not found to be properly fossilized, which would have to be the case had the objects been in the ground for long ages. The utensils—harpoons, handaxes, scrapers—are far less artistic than those in other Neolithic stations. Vayson de Pradenne and Abbé Breuil indeed contend that none of them could ever have been used.

Dr. Morlet and his chief supporter Van Gennep did their best to answer all these objections. The former at the height of the verbal battle-royal made a request for a governmental commission which was speedily granted, but when he found that a bitter anti-Glozelian, the well-known archeologist Capitan, was a member, he objected and the Commission was never sent. Eventually, at the International Anthropological Congress at Amsterdam an International Commission was formally appointed to investigate Glozel. The Commission consisted of Absolon, Director of the Archeological Museum of the State of Moravia, Bosch-Gimpera, Professor in the University and Director of the archeological work of Barcelona, the Abbé Favret, Forrer, Director of the Prehistoric and of the Gallo-Roman Museum at Strassburg, Miss Dorothy Garrod, member of the Royal Anthropological Institute and of the French Prehistoric Society, Hamal-Nandrin, Lecturer on Prehistory in the Museum of Liège, Peyrony, Director of the Museum of Les Eyzies, and Pittard, Professor of Anthropology in the University of Geneva. Absolon was prevented from taking part in the work of the Commission.

After spending three days at the site the Commission issued a unanimous report which was kindly sent to me by Miss Dorothy Garrod. This report states unequivocally that the articles are for the most part of recent manufacture and have undoubtedly been planted in the ground by some one whom

the Commission does not name; and that Glozel is neither prehistoric nor authentic. Vayson de Pradenne in a devastating brochure in which he declared the Glozel finds fakes, also accused no one by name but put the blame upon the Esprit de Glozel; in other words upon a fairy.

One might think that with the leading French, English and American scientists—Peyrony, Pradenne, Abbé Breuil, Sir Arthur Evans, Dussaud and, I believe, Professor MacCurdy, arrayed against Glozel, and with the destructive judgment of the International Commission, Glozel would cease from troubling the scientific and the lay mind. Though all due obsequies have been performed, Glozel refuses to remain in its sepulchre, and the literary battle continues. Dr. Morlet kindly sends me newspapers and pamphlets and a distinguished pro-Glozelian of Belgium, Professor Tricot-Royer, has just supplied me with his defense of Glozel which is particularly interesting because Professor Tricot-Royer was present during the visit of the International Commission.

What keeps Glozel alive? First we have the fact that when men take sides in print they are loath to recant, fearing ridicule—the more untenable their position, the more stubborn their resistance.

Secondly, six months after the International Commission's visit Dr. Morlet called together a Comite d'Êtudes consisting of Dr. Foat, Bayet and Tricot-Royer of Belgium, Reinach, J. Loth, W. Loth, Van Gennep, Dépéret, Ajcelin, Roman, and Audollent of France, and Soderman of Sweden, all sympathetic to Glozel. At their meeting they pronounced unanimously in favor of its genuineness.

Another reason is found in the attitude of a group of French and German scientists who are opposed to the traditional belief that "ex oriente lux"—that civilization is of oriental origin. The alleged Neolithic alphabet of Glozel and similar finds at Alvao in Portugal are grist to their mill.

In addition quasi-political factors have entered into the controversy—Fradin an obscure peasant, Morlet a provincial doctor without much influence have a definite appeal for the proletariat and for a large section of the press.

And finally, it must be remembered that the Academicians are not always right—they ridiculed Pasteur and Boucher des Perthes; even Koch and Lister met a similar fate in the beginning.

All these elements coöperate to keep the spark of life in Glozel. Within the past few weeks the publication of an exhaustive treatise by Dr. Morlet has been announced. This, however, I fear, can throw no new light upon the subject.

As a detective tale the story of Glozel remains unfinished and will remain so until a Sherlock Holmes discovers the person or persons who manufactured the articles and put them in the ground. What was his motive? How are we to explain the extraordinary industry that has fashioned five thousand or more articles, and how is it that he, the Esprit de Glozel, has escaped detection in a community of twenty-nine souls where everyone knows everyone else's business? Or how, if there are witnesses to the dark deed, can we explain an unbroken neighborly silence extending over a period of six exciting years?

THE CURRENT STATUS OF INTERNATIONAL LAW

By CLYDE EAGLETON

(*Read April 24, 1930*)

NOT unlike other legal systems of today, international law has lately become vastly interested in its own philosophy. As a law which deals primarily with that metaphysical abstraction which we call the state, its philosophical background has always been particularly luring to students; and in few fields of such enormous practical importance has there been so much abstruse and controversial reasoning. But today we are getting our feet upon the ground. International law is going to the doctors—doctors of philosophy, *bien entendu!*—to have its face lifted. Indeed, the process is far along; and though it may not be recognizable to some of its former companions and friends, to the younger generation it looks much more presentable, much more engaging.

I should like to present to you some of the changes which I see in it. It is an advantageous time for such a study. Jurists in all nations have been challenging long undisputed conceptions; a large number of cases have been tried in recent years, affording new precedents; above all, states have themselves taken an interest in ascertaining and consolidating the law of nations. The discussions and preparatory work of the Conference for the Codification of International Law which has just been sitting at the Hague offer material of a type never before available, from which we can extract the philosophical explanations of this law, in the words of the states themselves. Moreover, international affairs now look much more interesting to the average citizen, as certainly they are of more importance to states, in these days of international interdependence.

May I first say, by way of clearing out the underbrush, that it makes no difference to me whether you call interna-

tional law true law or not. So far as I can tell, no one knows what law is today; and this being so, the Austinian definition of law, the only real antagonist of the law of nations, is as much in the arena, as much forced to defend itself, as is international law. Indeed, I should go so far as to say that it has already been unhorsed! It contents me that there is a body of rules recognized by states to be binding upon them, and capable of being enforced upon them on occasion. It is called law by states in their official correspondence, and by courts in their opinions; claims are presented and argued upon it as law; courts interpret it as law in strict accord with judicial traditions. No state ever denies its existence or its cogency. Whether you call it law or not does not disturb me: that is a mere matter of terminology, perhaps only of nomenclature. Certainly, whether law or not, there are some rules to talk about; and I propose now to talk about them—and call them law!

You will recall that international lawyers have been divided into two schools: natural lawyers, now supposed to to be an extinct *genus:* and positivists, to whom the daily doings of states constitute law. There has always been, however, an intermediate group called, with uncertain fairness, Grotians, and with more justification, eclectics—those who found it necessary to draw from both morals and practice and who, consequently, each to the other, included the worst vices of both. Today, we are told, natural law is dead; and positive law is a collection of *a priori* deductions from the doctrine of sovereignty, by a school which calls any sufficiently repetitious policy of states law. What phœnix is rising from these sad ashes? I am not prepared to say what the total result will be; but I should like to investigate two or three of the reincarnations of international legal doctrines now in progress; and then attempt to sum up for you the greatly improved situation of what Grotius—more correctly than he is given credit for—called the *ius gentium.*

Most important of these doctrines, because of its implications, and its influence upon both domestic and international law, is that of sovereignty. It is to Jean Bodin that the

concept of sovereignty is usually ascribed; but it was the German Hegel whose anarchic influence prevailed until the metamorphosis of the twentieth century began. From his doctrine that the state is an absolute end in itself, Lasson concluded that the relations between states are more a matter of mechanics than of law: "two states are related to each other like two physical forces." And from this the conclusion followed easily that war is the ultimate judge between right and wrong, so far as two sovereign states are concerned.[1] This is the necessary and unavoidable result of the doctrine of sovereignty; but very few are those who are courageous enough to accept it.

It is an anarchic and destructive theory, leaving the state utterly irresponsible. It means that international law is not binding, nor is any specific rule binding, except insofar as the state gives its consent thereto. Such a situation being unacceptable to the average man, and being contrary also to practice, positivists sought to avoid it by assuming consent on the part of states newly entering the community of nations, and by further assuming a tacit consent to rules of law which might be developed after their admission. Jellinek, perhaps the greatest of German juristic philosophers, taught that international law is created through the auto-limitation of the state, that the state is higher than any rule of law, and that, consequently, each state may reject that law as freely as it had accepted it. "International law exists for States and not States for international law." Others, however, asserted that once the consent of a state was given, it could not be revoked, and that thereafter the state was bound by the rule.

Such a theory of auto-limitation is the only possible working basis for international intercourse under the theory of irresponsible sovereignty; but it is a manifest *petitio principii;* and modern writers have not hesitated to expose it. It is workable only under the assumption that states are bound

[1] No attempt is made at exhaustive citation. The most valuable reference book, in which most of the citations may be found, is H. Lauterpacht, *Private Law Sources and Analogies of International Law* (London, 1927).

because they have agreed to be bound. But why is such an assumption—the *Vereinbarung* of Triepel—binding? If there is nothing higher than the sovereign will, and law is dependent entirely upon that will, how can a state continue to be bound by a law which at a later time is contrary to that will? Again there must be an assumption, expressed in the rule *pacta sunt servanda;* but why can not the state withdraw its consent from even this fundamental rule?[1] At this point, positivists are pushed back irresistibly to the recognition of an objective rule, superior to the will of a state, and binding upon that state. Unless such an objective rule be recognized, anarchy must follow; but when it is recognized, the doctrine of sovereignty is annihilated. The doctrine of auto-limitation is, as Hobbes would put it, to make a thing depend upon itself. You can not lift yourself by your own bootstraps.

Positivist writers have stretched themselves to the point of casuistry to maintain the fiction of sovereignty; but few of them have been able to be consistent. Hall, the authoritative English writer, is ranked as a positivist, but the foundation of international law in his mind is "an assumption that States possess rights and are subject to duties corresponding to the facts of their postulated nature"—a hypothesis superior to and independent of the will of the state. Similarly, Oppenheim, who is dogmatic in his assertion of positivism, asserts that a newly recognized state must submit to the rules of the community which it enters, and cannot choose which rules it will accept.[2] Why not, if the state is sovereign, and the ultimate source of law? The answer is that states are born into the community, willy-nilly, just as are individuals. States are daily compelled to submit their municipal law, even their constitutions, to the superior force of international law, and to make reparation whenever this law is violated.

Modern writers—and many earlier ones—denounce an

[1] See Triepel, *Droit Interne et Droit International*, Academie de Droit International, *Recueil de Cours*, 1923, Vol. I, p. 83; J. S. Reeves, *La Communauté Internationale*, *ibid.*, 1924, Vol. II, p. 52; Sir John Fischer Williams, *Chapters on Current International Law and the League of Nations* (London, 1929), pp. 13–14.

[2] Oppenheim, *International Law* (London, 1928), I, p. 121; but see the valuable note by McNair upon the same page in contradiction.

absolute and irresponsible sovereignty, and the accompanying theory of auto-limitation which reduces it to practicality—and also *ad absurdum*. Professor Brierly, of Oxford University, says, "The advent of constitutional government really demanded a new theory of the nature of states to take the place of the theory of sovereignty, which, though approximately true in its time, had ceased to be a rational account of the changed facts." In the United States, Professor J. W. Garner, *doyen* of political scientists, speaks of the "double-faced creation of the jurists which bears the name of sovereignty," and characterizes it, in the words of Hobbes, as "the ghost of personal monarchy, sitting crowned upon the grave thereof." Earlier, the always sensible Westlake had said: "Those therefore who lay it down that it is only by virtue of its consent to international law that a State is bound by it, are obliged to infer that consent from incidents in which we may be sure that nothing of the kind was thought of when they occurred." Kelsen, of the young Viennese school, of which Verdross is also a leader, asserts that there is above the state a legal order which fixes the powers of the individual states.

Nor does the practice of modern states offer any support for the doctrine of sovereignty. Domestic law is constantly subordinated to international law, whether in court decisions or in state papers; it was also apparent in the answers of governments to the questionnaire sent out by the Preparatory Committee for the Codification Conference, which deny the Calvo Doctrine and even hesitate to justify violations of international law upon the ground of reprisals or self-defence. Unquestionably, sovereignty today has been replaced by responsibility. It can now mean nothing more than a relative independence, the sphere of domestic jurisdiction permitted to a state by an international law which regulates its birth, its death, its succession, its local jurisdiction, its treatment of aliens, and many other things.

In attacking the doctrine of sovereignty, I have asserted the existence of norms independent of, and superior to, the volition of the state; and thus we are led to another important

element in the renovation of international law. From the forgotten limbo of discarded doctrines, the idea of natural law again arises—surely the phœnix from the ashes!

It is not the natural law of two or three centuries back, a subjective and indeterminate law, such as Hobbes and Pufendorf and others taught—it appears in yet more ancient garb. We must go back to the ancient philosophers; back to Aristotle who, in the Nichomachean Ethics, said, *Jus naturale est quod apud omnes homines habet potentiam;* and to Cicero and Gaius who, differing from Ulpian and Justinian, said that the *ius gentium* and the *ius naturale* were one and the same: *quod vero naturalis ratio inter omnes homines constituit, id apud omnes populos peræque custoditur vocaturque ius gentium, quasi quo jure omnes gentes utantur.*

There has always been something behind stated law—a reservoir upon which judges could draw in emergency. Quite recently, in a New York court, Judge Cardozo, in cases involving the rights of the unrecognized government of Russia in our courts, was forced to admit that there were no precedents, and to fall back upon common sense and justice. To the earlier writers, this reservoir was *recta ratio.* It was unsatisfactory and was discarded; and positivism reigned supreme. But not even the positivists, as has been seen, found practice sufficient. The *nuda historiarum recitatio,* said Gentilis, is fruitless. Even in the heyday of positivism there were many, such as Heffter, Westlake, Bluntschli, or Lorimer, who would not have admitted submission to that school. Practice refused to be bound by it. The *compromis d'arbitrage* in many an international arbitration has instructed the arbiters to render their decision in accord with the principles "of justice, equity, and the law of nations"—a different thing, be it noted, from a decision *ex aequo et bono.* Today we have returned to the old *ius gentium;* and as sufficient evidence for that statement I quote to you from Article 38 of the Statute of the Permanent Court of International Justice:

> "The Court shall apply:
> 1. International conventions. . . .
> 2. International custom. . . .
> 3. The general principles of law recognized by civilized nations. . . ."

What is this last—these general principles recognized by civilized nations—but the old *ius gentium?*

The law of nature has returned; but it is no longer a ghostly and unsubstantial thing. It now has a substantial foundation in objective rather than in subjective principles. Positivism is not annihilated; it is merely returned to its own proper place. Its influence upon the new natural law is unmistakable. Like the old *ius gentium*, natural law now embraces principles which are actually in use—which may be judicially proved to be in existence in the various legal systems of the world. It has not as yet found a proper title: it may be "universal conceptions of law," to use Jellinek's phrase; or "original law" as Sir John Fischer Williams prefers; or "general jurisprudence," or "law of nature with variable content," as others name it. Many have contributed to revive it: the German Jellinek, whose aid was perhaps unintentional; Nippold and Triepel and Liszt in Germany; Phillimore and Westlake, and now Brierly and Williams and Lauterpacht in England; the new Viennese school, led by Kelsen and Verdross; and especially Louis Le Fur in France. Little attention has been paid to it in the United States, perhaps because we have never entirely rejected the idea of a natural law; but the names of Dr. James Brown Scott and perhaps Dean Pound may be mentioned.[1]

The result is wholly satisfactory. International law is no longer static, but is able to keep pace with changing social conceptions. It is now respectable for a judge to fill in gaps in the stated law by appeal to higher general principles. The positivist should be satisfied, since these principles must be proven in court as actually ascertainable in legal systems; his opponent should be satisfied, since he is no longer compelled to regard whatever happens to be the consistent policy or action of states as of moral and legal cogency.

[1] On the movement for natural law today there is a valuable article by Louis Le Fur, "Le Droit Naturel et le Droit Rationnel ou Scientifique," in the *Revue de Droit International*, 1927, p. 3. See also Lauterpacht, *op. cit.*, pp. 19–20; Williams, *op. cit.*, pp. 15–27. Many other names could be mentioned, such as Duguit, Krabbe, Pollock, P. M. Brown; and mention should be made of the Declaration of the Rights and Duties of Nations adopted by the American Institute of International Law in 1916.

I shall have time to consider only one more of the transformations now taking place in the law of nations; and then I shall try to summarize for you its present status. Hegelian sovereignty, it has been seen, can no longer be accepted; but its ramifications throughout international law, like Johnson grass in a field, have not yet been weeded out. One of the worst of its effects is the present position of individuals before international law. Though Aristotle's view that the end of the state is the well-being of the individual is still affirmed to be correct, and though political philosophers now teach that the state exists for the individuals and not the individual for the state, still, international law gives no indication of having ever been introduced to the individual human being. States only are subjects of that law; individuals are merely objects, in the same category as ships, or cows or pigs, or other chattels. A society of states in which man's chief end was to serve as cannon fodder could hardly be expected to consider individuals as other than tools to serve the wishes of the teleological state. Surely we have progressed beyond such a feeling by now. But it is still legally correct to say that the individual has duties, but to his state only; that he has rights, but they can be enforced only when his state makes his claim its own. If he does harm, his own state must suffer for it; if he is harmed by another state, his own state may—not *must*—collect damages for the injury done to the state in his person— and it may reserve all such emoluments to itself, if it wishes!

While this theory is dominant, it has never gone unchallenged. To Suarez, individuals were subjects of the law of nations as well as were states; and the founders of international law addressed themselves to individuals. Mr. Lauterpacht closes his valuable book with this statement:

"An ever-growing body of international legal opinion—from Westlake and Lorimer to Krabbe, Kelsen and Duguit—has been engaged during the past half-century in impressing upon the mind of the student and the statesman what the author believes to be a true and beneficent notion, namely, that behind the personified institutions called states there are in every case individual human beings to whom the precepts of international law are addressed. Thus interpreted, the exalted conception of the father of international law and of other publicists of the classical period which regarded

nations as moral individuals *respectu totius generis humani*, is still an ideal worthy of pursuance."

Today there are many who are willing to break a lance in behalf of that forlorn and forsaken creature, *genus homo*.[1]

But aside from theory, practice has never been subjugated to the doctrine that states only have rights and duties under international law. It is commonly admitted that many entities aside from sovereign states—if there be such things!—have some degree of international personality. What of the League of Nations, the Papacy, the Pan-American Union, the British Dominions, certain international public unions, the Danube Commission or the Reparations' Commission? Perhaps no one knows just what each one of these is; no more does one know how Cuba can be called a sovereign state, or why the Sultan of Johore is entitled to the immunities of a sovereign. As Lawrence says, the classificatory skill of political scientists lags far behind the constructive ingenuity of statesmen. But it is clear that not all so-called sovereign states possess the same degree of personality in international law; and that there are others than states which possess some measure of that personality.

Among these are individuals, who have never been completely ostracized, in spite of rude repulses. Pirates, blockade runners, contraband carriers, perhaps also slave-traders, have long been in possession of duties and rights under international law. And there are other acts of individuals which may violate the law of nations, such as counterfeiting, certain acts in violation of the laws of war, damaging submarine cables, crimes in *terra nullius*. It is no refutation of the above to say that the law violated by these individuals is always a state law—if this should be admitted there would be no international law. On the contrary, the state may act in such cases only under the authority of international law. The state can not define piracy; it can only act as the agent of

[1] Among these would be found most of those above listed as supporting a law of nature; and one may name also Heffter, Kaufman, Rehm, Nicholas Politis, Phillip Marshall Brown, and others. Valuable references will be found in Lauterpacht, *op. cit.*, pp. 77-79; and McNair's Oppenheim, p. 21, note 3.

the society of nations, and under its authority, in punishing a pirate. If international law is "part of the law of our land," as our courts say, as the newer constitutions of Europe more precisely say, how can it be denied that international law reaches the individual?

International courts have not been given jurisdiction over individuals except in one case; but strong tendencies have been shown in that direction. The Permanent Court of International Justice is daily forced to turn down the pleas of individuals; but the Committee of Jurists which created that Court contained a strong minority which favored such a jurisdiction. A Prize Court would once have been set up could prize law have been agreed upon; a Central American Court of Justice actually functioned for seven years and tried a number of suits of individuals against states; a serious study is now being made under the League of Nations of an international criminal court. The Danube River Commission applies its rules to individuals; the various *Tribunaux Arbitraux Mixtes* executing Article 297 of the Treaty of Versailles deal mostly with individual claims against states. Claims Commissions discuss claims as belonging to individuals at the very moment that they assert an individual can have no claim—that it belongs to his state!

Indeed, when one studies the pecuniary claims of individuals, the absurdity of denying their rights under international law becomes apparent. The whole of the international law for the protection of aliens is in existence for the benefit of individuals. The fiction is arduously maintained that his claim is the claim of his state. But it is only when the individual has been injured that the state can make a claim; the state is not permitted to make a claim if the individual ceases to be a national of that state—although, be it remembered, it is called the state's claim; damages are measured by the loss caused to the individual rather than by the loss caused to the state; and the money is awarded to the individual, though it is paid to his state.

It is all a ridiculous and unnecessary sacrifice of the

individual to the ancient god of sovereignty. All law, all political organization, exists for the benefit of the individual. "The duties and rights of States," said Westlake, "are only the rights and duties of the men who compose them." The state is merely the agency set up by the individuals who compose it, to care for their collective interests, and to represent them to the community of states, as it conversely represents the community of states to its own members. That it should be permitted to deny to its members the right to pursue their just claims against other states, or that other states should be permitted to evade such claims on the ground of sovereign irresponsibility, is not susceptible of reasonable explanation, and is merely a survival of an anachronistic doctrine. The state, I repeat, is merely an agency working for the benefit of individuals; and if the individual is compelled, as a matter of procedure and convenience, to submit to the interposition of his state, that is quite a different thing from saying that he has no rights, and that his claim belongs to his state. It is the fault of positivist thinkers that they have sought, by a process of deduction, to justify a preconceived doctrine of sovereignty, rather than, by a process of induction, to build a theory from the facts of practice, upon which positivism is founded.

If the changes which I have noted are realized, we have a much broader and sounder foundation for the law of nations—a foundation which will aid the individual and therefore interest him in the law. There has been some ground for his cynicism as to international law. It has been too much interested in the so-called fundamental rights of that artificial personification, the metaphysical state. It has not reached out to the more important matters of human concern. It has been incomplete and sometimes not clear; it has lacked any effective means of enunciating badly needed new law; it has been believed to lack the sanctions necessary for its enforcement.

All of these defects, I say now without undue optimism, are being remedied. With the doctrine of sovereignty dis-

carded, attention may be centered upon real human needs rather than upon the artificial claims of the state. With that doctrine removed, it becomes possible to legislate; and such of its corollaries as equality and independence, a *liberum veto* upon legislation, are rapidly disappearing as obstructions. Machinery for legislation is appearing. Improved conference methods, leading to multilateral treaties, are fast producing new rules. Official codification, in a legislative sense, is in progress, the first conference having been held last month. A means of revising treaties, not yet tested, is found in the Covenant of the League of Nations. In general, the League provides coherence, and a machinery long lacking in the international community; and its success thus far gives renewed confidence to that community.

With renewed courage, we are reaching out to more important problems, to those which humanity wishes above all to be solved. We have abandoned the hopeless effort to make war more humane, and are concentrating our efforts upon the control of war itself. It is significant to observe that the late textbooks of international law omit what previously occupied over half of those texts, the law of war. The Kellogg Pact lays down an obligation, unfortunately not put into practical terms, not to resort to war. Most of the states of the world have returned to the Grotian distinction between just and unjust war. They have abandoned neutrality as immoral, have adopted obligatory methods for settling disputes peacefully, and have even provided potential sanctions for use against aggressor states. The machinery of the League of Nations lacks only the coöperation of the United States, indeed it waits only upon the abandonment of our neutral rights, to make it an effective instrumentality for preserving peace.

Where such new methods of legislation are insufficient, we can now fall back upon the law of nature—upon the "general principles of law recognized by civilized nations," which the Permanent Court must apply. And if one is not willing to go so far as does Sir John Fischer Williams, who says

that there are now no gaps in international law, and that the judge has now at his disposal enough to enable him always to reach a decision, one may feel confident that in most cases the judge will be able to judge.

The rules of law are the product of necessity, thrown up by the resurgent pressure of mutual interests. They appear as society feels the need of them. If international law has been slow in developing, it has been because the pressure of interdependence in the society of nations has only been felt in the past century, and is only now reaching its high tide.

One last point I offer for your consideration. I am unable to answer it. Internally, in political organization, there has always been a superior authority, capable of enforcing its demands upon its subjects. Today, President Hoover announces as the policy of the United States that coercion is unnecessary in international affairs—that nothing more is needed than the suasion of public opinion. The rest of the world have, apparently, accepted the principles of the League of Nations, which offers means of coercion through the concerted, though voluntary, coöperation of its members. These means cannot be effective, which means that the principles of the League of Nations cannot be put into operation, without the coöperation of the United States against the aggressor. Thus the issue is joined. Which of the divergent paths thus marked out I do not know; whether the society of nations will proceed along historical lines of political development, or whether a new theory of political organization is in conception, I do not venture to prophesy. But of one thing I am sure: that no progress will be made in any direction, without consultation, without coöperation.

THE NATURAL CHEMICAL REGULATION OF GROWTH BY INCREASE IN CELL NUMBER

By FREDERICK S. HAMMETT

(*Read April 25, 1930*)

A YEAR ago it was my privilege to present experimental data leading to the conclusion that the —SH or sulfhydryl group is the essential chemical stimulus to growth by increase in cell number. At that time it was shown that this chemical group is naturally concentrated in regions where growth by cell proliferation is actively taking place, and that the process is inhibited when the —SH group is removed. Further it was shown that cell proliferation of simple organisms such as root tips and paramecium is accelerated under rigidly controlled conditions by extremely low concentrations of —SH; *i.e.* one part of sulfur as —SH in ten million parts of culture solution. Since both plants and animals responded the biological universality of the principle seemed established.

On the basis of these results it was predicted that if the postulate is true, the application of the principle should be of assistance in the healing of refractory sores and wounds in man. With the coöperation of Reimann this idea was put to test. It was found that the —SH group is an accelerator of healing of long standing ulcers, bed-sores and the like in patients incapacitated by such disturbances. I need hardly state that it has been most gratifying to observe the prediction come true and the useful application of this piece of pure biological research. Further, as reported by Reimann, even when no wound is present an active cell proliferation is produced in the skin of rats by the simple surface application of thiocresol, while cresol itself has no such stimulating effect when applied in equal concentration for equal periods.

The results with higher organisms establish beyond peradventure the general validity of the postulate that —SH is

the essential chemical stimulus to growth by increase in cell number.

Now it is well known that normal growth by increase in the number of cells is a limited process. Sooner or later cell proliferation as a participant in body size increment stops. If this did not occur there would be neither space nor food available for such continuously unfolding organisms. Something occurs, then, either in the external or internal environment of living things which puts a brake and brings to a stop their unlimited growth through cell multiplication.

Now there is a law of chemistry called the Law of Mass Action which states in essence that the progress of a reaction is gradually slowed and brought to a state of equilibrium by the products resulting from the reaction itself. The masterly analysis of growth curves by T. Brailsford Robertson combined with chemical knowledge of tissue changes with age has put beyond reasonable doubt the idea that the course of assimilatory growth increment, or growth by increase in cell size is governed by this fundamental law.

Since the processes underlying cell multiplication are likewise chemical, it is clear that the Law of Mass Action must also be in effect in this phase of growth. The course of growth by increase in cell number is but an expression of the working of this law. In other words growth by cell division is regulated by a chemical equilibrium. The problem, then, resolves itself into a determination of the specific chemical groups and reactions responsible for the equilibration.

This postulate demands that the specific chemical regulatory factors can only be naturally occurring chemically related groups in which the inhibiting members are reaction product derivatives of the accelerating.

That is to say, from the point of view of normal growth, any natural accelerator must be the antecedent of the inhibitor through normal organic reaction by the organism; and any natural inhibitor must be the derivative through normal organic reaction of the natural accelerator. Our problem, then, concerns itself solely with the natural chemical equilibrium regulating growth by increase in cell number.

Now it is known from studies on higher organisms that a large part of the sulfur of living tissue is in a reduced or relatively reduced state, as in glutathione representing the R−SH type, and in cystin representing the R−S · S−R type of linkage. On the other hand the sulfur as excreted by the body is largely in the completely oxidized state or in the form of sulfates. Obviously this oxidation does not take place in one fell swoop but is an orderly step-by-step process as are all physiological oxidations.

The several steps in the oxidation of an R−SH compound can be represented as follows:

$$R-SH \rightarrow R-S\cdot S-R \rightarrow R-SOH \rightarrow R-\overset{\overset{O}{/\!/}}{S}-OH \rightarrow R-\underset{\underset{O}{\backslash\!\backslash}}{\overset{\overset{O}{/\!/}}{S}}-OH$$

Since the reduced or −SH state is the essential stimulus to growth by increase in cell number, it would be expected, if the reasoning given above be correct, that the last stage of complete oxidation would have no action; since excretion removes this rapidly; but that somewhere in the oxidation chain from −SH to SO_4, partially or sub-oxidized compounds would be produced which are inhibitive of cell multiplication. Other possibilities, even probabilities are present, but these need not be gone into in detail here. Suffice it to say that theoretically the final course through the penultimate or sulfinate stage is unavoidable, whether the metabolic process is one of direct oxidation, or one of combined oxidation and hydrolysis.

The past year has been occupied with the testing of this hypothesis. The ground work was done with root tips of Zea Mays, as in the previous studies, since the rate of mitosis can be conveniently and accurately determined by nuclear counts in serial sections of this material after subjection to one and another experimental procedure.

The cell proliferation reaction to a series of compounds in which sulfur is partially oxidized was compared with that taking place when the same root compounds but containing

the sulfur in the completely oxidized state were used in control cultures of like pH and like sulfur concentration.

The simple inorganic compounds, sodium bisulfite and sodium hydrosulfite were first tested, using equivalent concentration of sodium sulfate as control. Cell division was uniformly retarded by sodium bisulfite in concentrations as low as 2.5 parts per million added sulfur.

Now sodium hydrosulfite is a peculiar compound in that it is reduced in acid solution and H_2S is produced, while in alkaline solution no such change is exhibited. Hence it would be expected that root-tips grown in acid cultures containing this compound would show increased mitosis, since H_2S is a sulfhydryl compound stimulative of cell multiplication as shown by Miss Sharpe. Those grown in alkaline solution should on the other hand show a decreased division rate. Such occurred. This fact constitutes conclusive proof of the validity of the hypothesis that growth by increase in cell number is regulated by a chemical equilibrium in which $-SH$ is the key group. For with one and the same initial sulfur compound stimulation or inhibition of mitosis can be obtained at will depending upon whether one presents it in the reduced state in acid solution, or in the semi-oxidized state in alkaline. This is of the utmost biological significance.

Having no desire, however, to base my thesis on results from one or two compounds I next compared the action of a series of organic compounds containing partially oxidized sulfur groups with their control compounds of the same composition save that the sulfur was in the completely oxidized state. Sulfinates, sulfoxides, and sulfone were tested against their sulfonates in equivalent concentrations with respect to sulfur, and in solutions of the same pH. In every case the sub- or partially oxidized sulfur group exerted a differential retardation of growth by increase in cell number.

It should be noted in passing that root length growth was also retarded. This was not due to decrease in cell assimilation growth since cell size in the shorter test roots was greater than in the longer controls. It is therefore clear that the

lesser root length growth was due to the fact that fewer cells were formed during the period of observation. Thus there is double proof of the cell proliferation retardation effect of the sub-oxidized sulfur group.

So much for the plants.

As in the growth stimulation studies so here have I turned to paramecium as suitable material of animal nature for the testing of the influence of sub-oxidized sulfur compounds on cell proliferation rate. The methods of experimentation have already been described.

It was uniformly found that sub-oxidized sulfur in concentrations from 1 to 2 parts per ten million of culture definitely retarded the rate of reproduction of these unicellular animals. Rigid controls were simultaneously run in which the pH and S-concentration of the control completely oxidized sulfur compounds were the same as in the test cultures.

Thus in animals as well as plants growth by increase in cell number is retarded by the oxidized reaction product derivatives of the $-SH$ group.

While these results satisfy the conditions postulated for the establishment of the substances concerned in the chemical equilibrium specifically regulating growth by increase in cell number, they were obtained on relatively simple organisms. It seemed well therefore to extend the observations to higher forms.

The transplantable growth frequently occurring in mice was found to be a most convenient material, not only because large numbers of inoculated animals could be used, but also because the growth is one in which cell proliferation plays a major part.

Three experiments were run. In each the animals were divided into two lots, one for test and one for control, so that the distribution of the initial variation in size of growth was even. Completely oxidized sulfur compounds (sulfonates) were injected into the growths of the controls at suitable intervals, and sub-oxidized compounds (sulfoxides and sulfones) into the tests. At the end of the period of observation

the growths were removed and weighed individually. In each of the experiments the average weight of the material in contact with sub-oxidized sulfur was less than that of the control.

Further there appeared two mice in our colony with spontaneous growths. One of these regressed entirely, while the other was patently slowed in its development under the action of sub-oxidized sulfur.

These results are consistent with those obtained on other biological material and thus allow extension of the principle from the lower to the higher organism.

It should be mentioned here that the data as a whole are not drawn from three or four experiments, but from counts of something over a million nuclei, and close to a hundred thousand paramecia. The cumulative material is ample proof of the validity of the changes observed.

To summarize.

Growth by increase in cell number is the expression of underlying chemical reactions. As such it is subject to the laws governing chemical reactions. Of these the Law of Mass Action takes precedence because this law is a statement of the equilibrium conditions of chemical reactions and because growth by increase in cell number is obviously an equilibrated process. That is to say, this phase of growth is governed by a chemical equilibrium. The problem then resolves itself into the determination of the chemical compounds concerned in this. From the foregoing it is axiomatic that no chemical system can be considered as the natural regulatory system, unless the stimulatory member is the chemically related antecedent of the naturally occurring inhibitory member through organic reaction. Conversely the inhibitory member must be the chemically related reaction product derivative of the natural stimulating group.

Through step-by-step experimentation with plant and lower and higher animal forms, it has been found that the $-SH$ or sulfhydryl group is stimulative specifically of cell division and that its sub-oxidized organically produced

derivatives are inhibitive. Both the reduced and the sub-oxidized states of sulfur are natural components of living tissue, and living tissue induces the change from the reduced to the oxidized forms. This system satisfies the conditions laid down above. Hence it is concluded that the $-SH$, incompletely oxidized $-SH$ groups comprise the chemical elements of the naturally occurring chemical equilibrium through which growth by increase in cell number is regulated.

It must be emphasized that the concept lies in the chemical groups, not the radicles to which they are attached. It is the groups which are the labile effective members in the equilibrium, not the radicles. This is shown by the known changes which take place in the $-SH$ group and also by the fact that the characteristic responses of cell proliferation are obtained whether the groups be attached to inorganic, or aromatic or aliphatic organic roots.

I need hardly go into the potentialities inherent in this finding. They should be obvious to all. The possibility is opened up of governing at will through intelligent application of this exact chemical equilibrium, all those processes which affect mankind through the phenomenon of cell proliferation or reproduction.

RECENT ARCHÆOLOGICAL DISCOVERIES IN THE PHILIPPINES AND THEIR BEARING ON THE PRE-HISTORY OF EASTERN ASIA

By R. B. DIXON

(*Read by title April 26, 1930*)

UNTIL four years ago, the archæology of the Philippines back of the period which may be placed roughly about the year 1000 was a complete blank. There had been found in burial caves mainly in the Visayan portion of the archipelago, human remains associated with Chinese porcelain vessels, dateable as far back as the Sung Dynasty. Especially large and systematic collections of this material had been made by an expedition sent out by the University of Michigan under Dr. Guthe, which had worked during the period from 1922 to 1925. But the caves had yielded no materials dating back of the era of Chinese trade, and except for a score or so of stone implements found at various times in various parts of the islands, mostly in use as charms or amulets by the people, no finds dating to prehistoric times were known.

Early in 1926, in the course of the construction of a dam on the Novaliches river, built to form a reservoir for the water supply of Manila, a prehistoric village and cemetery site was discovered. The find was brought to the attention of Professor H. Otley Beyer, head of the Department of Anthropology at the University of the Philippines who, at once realizing its importance, began a systematic investigation of the site. It was necessary to work rapidly, as the steam-shovels and engineering work at the dam threatened to destroy the whole deposit. In the following three or four months Professor Beyer secured something over eighteen thousand specimens, representing five successive archæological horizons.

The uppermost contained Chinese celadons, porcelains and other ceramic wares, dating back to the early Sung Dy-

nasty, duplicating thus, the finds previously made in the burial caves of the islands south of Luzon. Below this came two strata belonging to the Iron Age, and below these again were two purely stone age levels. At one stroke thus, the history of human culture in the Philippines was carried back at least to early neolithic times.

Encouraged and enthused by this beginning, Professor Beyer after the work at Novaliches had come to an end, began an extended reconnaissance of the region round about, and in the course of the next three years, had located nearly a hundred sites, and made at each of them excavations of varying extent. Having been unable to enlist any government or private support, Professor Beyer has carried on the entire work at his own expense.

The horizons discovered first at Novaliches were found again and again, and from his work, which lay almost wholly in the province of Rizal, Professor Beyer has now at his disposal an enormous mass of material totalling scores of thousands of objects. Obviously it will require a long period of study to work out all the results, but certain broad conclusions may already be outlined. These were presented in a brief paper contributed by Professor Beyer at the Third Pan-Pacific Science Congress at Tokyo in 1926. During the past summer it was my good fortune to be in Manila and to have the opportunity of going over with Professor Beyer, a large portion of his collections and discussing with him the conclusions he had reached and their bearing on the pre-history of eastern Asia. My purpose in this brief paper is to call attention to the great value of the work done, and to outline some of the conclusions to which it leads. I have no personal contributions to make, except perhaps in pointing out one or two of the implications which these finds involve.

In his preliminary paper, Professor Beyer recognized two distinct neolithic horizons, the later characterized by well polished implements associated with fairly good handmade pottery; the earlier only partly polished and associated with a

cruder pottery type. The work done since in his wider reconnaissance of the whole province of Rizal, has revealed two others. The first of these, lying below the stratum just mentioned, has implements which are chipped only, not ground or polished, deeply patinated in all cases, and typologically identical with the so-called "Bacsonian" forms found by the French in Tonkin. No pottery is associated with these, and the evidence at hand, both stratigraphic and typological, would justify us in classing them as mesolithic. Whether it is justifiable to regard the other new type of implements, which are typical microliths, as older yet, and really palæolithic, is still uncertain. In this case there is no stratigraphic evidence, but these and some deeply patinated Mousterian-like forms, certainly suggest that conclusion.

The definitely neolithic forms have as a result of the more extended excavations, revealed a larger series of distinct types than was at first supposed. The most notable results are in my opinion, that several of these types show close analogies with the more characteristic Melanesian and Polynesian forms, and that the shouldered celts so typical of south-eastern Asia, related forms of which occur in southern Indonesia and Polynesia, are practically absent. In view of the relative abundance of these shouldered celts on the Chinese coast in the vicinity of Hong-kong, their rarity in the northern Philippines is certainly striking. The incompleteness of our data on the prehistory of all Indonesia and Melanesia and the fragmentary information which we have for most of south-eastern Asia, make detailed conclusions in regard to these neolithic finds still premature. But they seem to indicate at least, that archæological verification of the various racial and cultural drifts that have passed from Asia out into the Pacific, is going to be possible so soon as intelligent effort is devoted to excavation.

It is from the finds in the Iron Age strata which overlie the neolithic deposits, that immediate conclusions can legitimately be drawn. These comprise pottery of a considerable range in quality and types of decoration and a very large

variety of forms. Secondly, iron implements and weapons, such as knives, axes, daggers and spear-points. Thirdly glass beads and bangles, both green and blue, and finally, beads of semi-precious stones such as agate, carnelian, amethyst and rock-crystal. It is certain that some at least of the iron objects were of local manufacture, since deposits of iron slag and evidences of iron smelting have been found. It is uncertain as to the glass, but unfinished beads adhering to each other in series of half a dozen or more are found, and clear evidence of the repairing of broken bangles. In the earlier Iron Age strata only green glass, whose color is due to iron, occurs; in the later, both this and a blue glass whose color is due to copper.

Now both the iron and glass objects are similar to and in some cases identical with, the prehistoric glass and iron finds in the south of India. These occur in the dolmen tombs and urn burials which are found by the hundreds of thousands, and which almost certainly antedate the historic Chera, Chola and Pandyan kingdoms, whose history goes back to the beginning of the Christian era or before. As finds of similar glass beads and bangles have recently been made in the Malay Peninsula, in dolmen tombs in Java, and in North Borneo, the inference is inescapable that we have clear evidence of a trade contact between the northern Philippines and southern India, running well back into the first millenium B.C. The extensive trade and colonization and later conquests of the south Indian kingdoms, in Sumatra and Java as well as in Indochina in the early centuries of the Christian era, are of course well known. This new material, however, seems to make it clear that this was far from being the beginning of such contacts, but rather the last stages in an association reaching as far as the northern Philippines, which had begun many centuries before. In Chinese historical sources, there are a few references to maritime traders bringing typical Indian products to China as far back as the 7th century B.C. These accounts have generally been regarded with increduilty or strong suspicion at least. In view of this evidence from the Philippines the probability of these accounts is greatly increased, with con-

sequences for the history of Chinese culture which are obvious.

A whole new chapter seems thus to be opening in the early history of southeastern Asia and Indonesia. So little serious attention has yet been paid to the pre-history of the whole of southern India, that the course of its development and the origins of its culture are still virtually unknown. That the knowledge of glass-making reached it from western Asia is extremely probable, either by way of the sea-trade with southern Arabia, Mesopotamia and Egypt, or possibly overland. Thus southern India becomes a way-station between western Asia and the Philippines in the diffusion of one cultural trait at least.

Of the sherds of Chinese ceramic wares which occur in enormous quantities in the strata overlying those of the Iron Age, I need say but little. Their importance lies in their stratification. The finds of Chinese porcelains in the burial caves of the southern islands show no stratigraphy, so that they are undateable except so far as the period of their manufacture. There is no evidence as to when they may have reached the Philippines. Professor Beyer's finds, however, will enable us to determine much more in this direction. Further, he believes that some of his sherds are undoubtedly of wares made in the later T'ang Dynasty, and thus older than the hitherto recorded finds in the burial caves.

With the discovery of the ancient cultures of the Indus Valley at Mohendjo-Daro and Harappa, a new era in our knowledge of the origins and development of Indian culture has been begun; with the recent archæological discoveries of Dr. Anderson and Dr. Li in China, we for the first time have begun to get a glimpse into the early stages of growth of Chinese culture. The finds made by Professor Beyer during the last four years in the Philippines, have similarly opened up for us a wholly new vista, which not only carries our vision in one sweep back perhaps to palæolithic times, but shows us clearly that even this remote fringe of the Old World was reached by cultural streams, some of whose sources lay in western Asia, and whose influence was felt here perhaps as early as the beginning of the first millenium B.C.

THE TOXICITY OF MOLECULES AND IONS

By RODNEY H. TRUE

(*Read by title, April 25, 1930*)

What is Toxicity?

In seeking to define what is meant by toxicity, I find it not easy to do it in such a way as to exclude what we call malnutrition and starvation. When a culture medium, for example, lacks only calcium the plant needing that element will dwindle and die. We may say that the lack of Ca did the deed, but it seems to me to be an unsatisfactory way to view the situation, true though the statement may be. We might, with like logic, blame the police department for a robbery because the protecting factor, the policeman, was not there. The robber was there and was able to do his job because of his own activity. Thus the elements of a culture medium have harmful capabilities in them if they are not balanced off against each other. Hence the culture medium, perfect had Ca been present, develops toxic properties in its absence and slow murder happens. Therefore, it is difficult to define toxicity, and perhaps no sharp line can be drawn around it.

Still, the idea associated with toxicity remains with us and perhaps may denote an active interference with the normal chemical work of the organism bringing about a more or less complete derangement of things and perhaps death. Something cataclysmic seems to inhere in the notion. The ill-adjustment of parts in a machine or the lack of lubricants may result in time in its wreckage, like the lack of the Ca ions in our plant, but the proverbial monkey wrench thrown into the machine would afford a better analogy for toxicity as we usually think of it in connection with living things.

In this discussion, however, we shall not be troubled by fine distinctions since the criterion by which we shall judge toxicity is the approach of that irreversible reaction called *death*.

It is a matter of common knowledge that a living thing can be killed within a more or less definite time by the violent chemical influence of molecules and ions.

Antiseptics, Aseptics and Sprays

We have long known in a practical way the, to us, useful efficiency of toxic substances. Pathogenic organisms living in men or in our crop plants have long been fought by the use of toxic agents, the so-called antiseptics, aseptics and fungicides and a variety of such substances are well known to doctors, farmers and the rest of us. They consist usually of substances not found in solution about us and we associate them with the trouble that brings them into use. Among others we may recall the solutions of iodine, mercury compounds, silver used as the nitrate or restrained in its action in such preparations as argyrol and arkase. Copper, arsenic and lead salts are at the basis of important fungicides, algæcides and insecticides.

Studies on Higher Plants

It has been my good fortune for some years to be associated with colleagues and students in a study of the toxic activity of a long list of inorganic and organic compounds. We used the white lupine, *Lupinus albus*, as the test plant. I wish to bring before you in concise form some of the results obtained in those studies.

Toxic Equivalents

It was thought, since substances have their combining ratios and characteristic chemical properties, that there might be a unit of a biological character for each substance, a *toxic equivalent*, that might have both chemical and biological interest, and it was even thought that perhaps this biological equivalent might be found to stand in some suggestive relation to one or more of those constants expressing physical or chemical properties in the substances tested.

Method of Finding Toxic Equivalents

It was of course important to make up our solutions in such a way that they might be directly comparable as to concentration. Consequently stock solutions were prepared on the molecular basis. The chemical formula of the compound being known the sum of the atomic weights was determined in grams and the substance dissolved in distilled water sufficient to make a liter of solution. This stock, $M/1$ or fractional dilution of it, was used to give the concentrations to be tested and results involving concentrations are stated in *gram. mol.* per liter. It is more convenient usually to state it in the number of liters in which one *gram. mol.* of the substance is dissolved. Thus in this paper $M/6400$ is stated simply as 6400.

To get plant material, seeds of lupine were germinated in moist sphagnum to a point at which the seedlings had radicles 3 to 4 cm. long. The root was then provided with an India-ink line 10 mm. back from the tip to make it possible to measure growth. The seedlings were supported in beakers containing 400 cc. of the solution to be tested with the roots in the solution. Daily observations were made and the results noted. The *toxic equivalent* of a substance was the weakest concentration in which the roots were able to survive for 24 hours. Survival was determined by persisting turgor of the root, with some growth during the second 24 hour period.

Toxicity of Common Inorganic Acids and Na Salts

These studies began with a series of tests of the common *inorganic acids* and their *Na salts*.

TABLE I

Toxic Equivalents of Inorganic Acids and of their Sodium Salts

Acids	Toxic Equivalents	Toxic Equivalents	Sodium Salts
HCl	6,400	16	NaCl
HBr	6,400	16	NaBr
HI	6,400	16	NaI
HNO_3	6,400	16	$NaNO_3$
H_2SO_4	12,800		
$KHSO_4$	6,400		

It seems that these acids are about equally toxic in chemically like quantities and that the harmful component is the thing they have in common, the H ion. It will be noted that when two H ions are present as in sulphuric acid, the toxicity is doubled. Thus it appears probable that the H ions are so much more harmful than the anions accompanying them that the influence of the latter becomes practically negligible; also that the influence of these different anions is strikingly similar.

In the hope of getting light on this question, sodium salts of these acids were tested with the results also seen in Table I. It is to be noted that again the toxic equivalents agree as would be expected in view of the occurrence of Na in all molecules, but the toxicities of the H and Na ions are seen to lie very wide apart. The H ions are about 400 times as toxic as Na and Cl ions together. Since we cannot establish the toxic equivalent of any one ion apart from the presence of another, we may get an approximate basis for calculating the toxicity of a series of ions by assuming that the toxicity of the Na ion is less than 16, the same value holding for Cl by the same line of reasoning. If then the anions Cl, Br, I and NO_3 have a toxic equivalent <16, the equivalent for H is about 6400. This is borne out by the behavior of H_2SO_4 and its acid K salt. The toxic equivalent of H_2SO_4 is twice that of the salt in which one $\overset{+}{H}$ is replaced by $\overset{+}{K}$ and incidentally, the toxic equivalent of the $\overset{+}{K}$ is shown to be so small as to be negligible, like the $\overset{+}{Na}$ ion.

Since, then, the toxic equivalent of the commoner inorganic acids is due to the H ion almost completely we are in a position to learn about the other electro-positive ions. A series of studies was made with the salts listed in the following table (Table II).

I should like to call your attention to a number of points in these two groups of salts. You will note that there is little correspondence between the atomic weights and the toxic equivalents. Since the action of the cations has been shown to be predominant in cases, a number of physical properties

TABLE II

TOXIC EQUIVALENTS OF ALKALIES AND OF ALKALINE EARTHS

Salts	Atomic Weight	Speed of Ions	Solution Tension	Toxic Equivalents
		ALKALIES		
LiCl.........	6.9	33.4	−3.305	256
NaCl........	23.0	43.5	−2.993	16
KCl........	39.1	64.	−3.203	16
RbCl........	85.4	67.5	−3.203	48
CsCl.........	132.8	68.0	—	100
		ALKALINE EARTHS		
$BeSO_4$.......	9.1	28.0	—	6250
$MgCl_2$.......	24.3	($\frac{1}{2}$) 45.0	−1.55	750
$CaCl_2$........	40.1	($\frac{1}{2}$) 51.0	−2.55	3.9
$SrCl_2$...... ..	87.6	($\frac{1}{2}$) 51.0	−2.767	3.3
$BaCl_2$	137.4	($\frac{1}{2}$) 55.0	−2.7	3333.

of these cations are given for comparison with the toxic equivalence. The atomic weight is a most important constant that runs through all chemistry. The relative speed of the cations as determined by Kohlrausch is indicated showing the rates at which they migrate through aqueous solutions toward their proper electrodes. The number of electric charges on a given ion corresponds with the number of H ions it can replace, 1 in case of most of the ions we have so far studied and represents the *quantity factor* of an ion in doing chemical work. The tendency of the ion to give up its charge or the eagerness to combine represents its *intensity factor*, its solution tension. Ions, having a like electric charge or capacity factor, do not then necessarily act alike in view of the inequality of their ion voltage, expressed as solution tension. Although solution tensions are differently indicated by different investigators, I have here used the values given by Mc Clendon in his "Physical Chemistry of Vital Phenomena." The toxic equivalents given have been determined by a number of us. Since in all cases the numerator is 1, the toxicity is measured by the denominator only which indicates the number of liters of

solution in which one molecular weight in grams of the given substance is dissolved to give the toxic equivalent.

An inspection of Table II brings out certain features. The most toxic cation of the alkalies has the smallest atomic weight, the next most toxic has the greatest, the very slightly toxic nutrient ions, Na and K occupying the middle position.

Among the alkaline earths, beryllium, the lightest, is by far the most toxic, the next in order again being Ba with the greatest atomic weight, the least toxic ions, Ca and Sr being intermediate.

The speed of the ions is likewise not at all paralleled by the toxicity in either group. The solution tension ratios also offer no parallel.

The biological response of lupines to these solutions presents no close reflection so far as I see of any of these usual physical or chemical properties of the substances concerned.

Heavy Metals

Turning now to the cations having heavier atomic weights, I have listed the salts tested in the order of the atomic weights of the electro-positive elements.

TABLE III

Toxic Equivalents of Inorganic Salts of Heavy Metals

Heavy Metals	Atomic Weights of Cations	Speed of Cations	Solution Tensions	Toxic Equivalents
$FeSO_4$	55.8	(½) 45 (⅓) 61	−0.43	25,600
$CoSO_4$	58.9	(½) 43	−0.29	25,600
$NiSO_4$	58.7	(½) 44	−0.22	25,600
$CaCl_2$	63.6	(½) 46	+0.34	25,600
$ZuCl_2$	65.4	(½) 46	−0.760	19,230
$AgNO_3$	107.9	54.3	+0.8	409,600
$CdCl_2$	112.4	(½) 46	−0.40	138,888
$HgCl_2$	200.6		+0.86	12,000

The atomic weights again seem not to keep step with toxicity. The relatively light Fe ion is twice as toxic as the heavy Hg ion, while Ag and Cd are most toxic. The speed of migration is hardly paralleled although the extremely toxic

Ag has the markedly highest rate of migration in the group. The solution tensions of the alkalies and alkaline earths were regularly low, those of the heavy metals not departing far from zero in either the positive or negative direction. The general statement made by Mathews (Physiol. Chem. 7th ed.) is true that great toxicity lies at one end of the scale of solution tensions, but it is also true that the toxicity curve by no means parallels that of solution tensions in several details, with the heavy metals, alkalies, or alkaline earths.

Anion Toxicities

Thus far we have dealt with cations only and we shall now deal with some of the commoner inorganic anions. Our first experiment shows that these are a rather harmless group generally speaking and any differences among them would have to be pretty striking to show against the high toxicity of the heavy metals or of the H ions of acids. Consequently to test the anions it is necessary to study them in compounds having the least harmful cations. As such, Na, K, Ca and Sr are prominent. Unfortunately there is no very complete series of data available but some light may be gained from tests made by Miss May Randall on the halogen salts of the alkalies and by Miss Mary A. Russell on some of the alkaline earths.

TABLE IV

Toxicity of Inorganic Anions

Salts	Speed of Anions	Solution Tension	Toxic Equivalent
NaCl	65.5	+1.35	16
NaBr	67.0	+1.08	16
NaI	66.5	+0.54	24
NaFl	46.6	+1.90	384
KCN			3200–6400

The generally weak toxic properties of these anions are well shown here. The Fl ion seems to be much more harmful than any of the other halogen ions, while I is only slightly more toxic than Cl and Br. KCN does not dissociate as freely as

the others in the series and the toxic equivalent may be due to the combined action of cations, anions and undissociated molecules. The cation action is <16.

On the basis of their general standing in the solution tension series, we might expect medium toxicity, considerably greater than that of the cations in the alkalies and alkaline earths. Such, however, is not the case, since in this respect these ally themselves in general with the nutrient ions of the alkalies and alkaline earths.

Much might be said about the manner of dissociation of double salts in relation to their action on plants. One or two instances must suffice. We have seen that the toxic equivalent of the Cu ion is about 25,600. When $CuSO_4$, sucrose and KOH react, a compound is formed that on solution in water ionizes in such a way as to split off K ions, leaving the Cu tied up in the complex anion. In solutions of this composition, the lupine roots were still living after twenty-four hours in the presence of the equivalent of 200 $CuSO_4$, the toxicity having been reduced to less than 1/64 by this treatment of the Cu. Several similar cases were worked out with other harmful ions, among others the CN ions. The toxic equivalent of KCN is a little more than 3,200, that of both K_4FeCN_6 and K_3 $FeCN_6$ was 200, 1/16 that of the ionizing compound.

Organic Acids and their Na Salts

We must not fail to give attention to some of the organic acids and their Na salts since here the situation is quite different in some particulars from that seen in the inorganic compounds. These differences are in part due to the tendency to dissociate into ions less freely, and perhaps in part to the large size of the anions. As molecules become less stable other dissociations than those discussed under the name of electrolytic dissociation probably take place, followed in some cases perhaps by electrolytic dissociation of the resultant products.

I would like to call your attention first to a series of acids and their Na salts. Tests were run with lupines on the homol-

ogous series of the simple fatty acids. Conductivity studies have shown that dissociation is not complete at concentrations permitting the growth of lupine roots, hence in accounting for a given toxic equivalent it is often necessary to reckon with three factors all acting at the same time: Cations, anions, usually more toxic than the common inorganic ones, and a greater or smaller number of residual molecules. Perhaps it is possible to approximate the value of these components. In the following table, the toxic equivalent is given and an attempt has been made to distribute the responsibility for the result among the three components named. In doing so the toxic equivalent of the Na ion is set down at <16. Since that is the toxic equivalent of Na and Cl ions in NaCl, either ion may be regarded as having a value <16. In view of the higher equivalents found for other components, the error that is clearly present in this assumption is relatively negligible. The number of such anions is found from the degree of dissociation of the Na salt and the toxicity of the anion can be approximated in this salt. Knowing these rough values, the degree of dissociation of the acid, the toxic equivalent of the $\overset{+}{H}$ ions present that of the anions can be calculated. This sum subtracted from the toxic equivalent of the acid gives the partial toxicity of the residue of undissociated molecules.

It is recognized that these values are very approximate at best but I offer the result in Table V.

TABLE V

Toxic Equivalents of Fatty Acids and of their Na Salts

Substances	Toxic Equiv.	Part Tox. Cations	Part Tox. Anions	Part Tox. Molecules	Ionization at Tox. Equiv.
Formic acid. . . .	6,400	4,480	36	1,884	70%
Na formate.	50	<14	>36	?	90 "
Acetic acid	1,600	960	11	629	15 "
Na acetate.	25	<14	>11	—	85 "
Propionic acid.	3,200	1,088	36	2,076	17 "
Na propionate . . .	50	<14	>36	—	88 "
Butyric acid . . .	3,200	1,152	36	2,012	18 "
Na butyrate	50	<14	>36	—	87 "

It appears that formic acid dissociating more freely than its homologues is more toxic than they on account of the partial-toxicity due to H ions, a considerable effect being exerted by the undissociated molecules. The anions in this whole series of acids like the inorganic anions seen above are but slightly toxic. In acetic acid, with low dissociation and lower toxic equivalent, the major part of the harm is caused by the $\overset{+}{H}$ ions. With propionic and butyric acids, having a low degree of dissociation and a higher degree of toxicity, the major partial-toxicity lies with the undissociated molecules.

It would take us too long to describe the work done on a long series of aromatic acids and their sodium salts, hence I will present a similar series of findings for representative compounds only.

TABLE VI

Toxic Equivalent of Aromatic Acids and their Na Salts

Substances	Toxic Equiv.	Part-Tox. Cations	Part-Tox. Anions	Part-Tox. Molecules	Ionization at Toxic Equiv.
Benzoic acid	6,400	2,432	185	3,783	38%
Na benzoate	200	<16	>184	—	92 "
Salicylic acid	6,400	6,016	85	300	94 "
Na salicylate	100	<16	>84	—	91 "
M. oxybenzoic acid	3,200	2,560	36?	600?	40 "
Na M. oxybenzoate	50	<16	>34?	—	—
P. oxybenzoic acid	3,200	1,600	34?	1,564?	25 "
Na P. oxybenzoate	50	—	34?	—	—
O. nitrobenzoic acid	6,400	<6,400	9–36	—	100 "
Na O. nitrobenzoate	25–50	—	9–36	—	85 "
M. nitrobenzoic acid	12,800	6,400	6,400	—	100 "
Na M. nitrobenzoate	400	<16	>384	—	—
P. nitrobenzoic acid	12,800	6,400	6,400	—	100 "
Na. p. nitrobenzoate	400	<16	>384	—	—
Protocatechuic acid	3,200	1,792	35?	1,423?	28 "
Na protocatechuate	50	<16	34	—	—
Gallic acid	6,400	2,240	85?	4,075?	35 "
Na gallate	100	—	85?	—	—
Cinnamic acid	12,800	2,880	785	9,135	45 "
Na cinnamate	800	—	785	—	96 "
Hippuric acid	6,400	1,920	10?	4,470?	30 "
Na hippurate	25	—	10?	—	—
Carbolic acid	400	00	00	400	00
Na carbolate	400	—	—	—	—

This table shows a range of toxicity to lupines in cases somewhat above and in others below that of HCl, apparently owing to the toxic properties of $\overset{+}{H}$ ions that dissociate off and a substantial part usually due to undissociated molecules. The anions are usually large in size, migrate slowly and are rarely of importance as toxic factors. In some cases changes not here accounted for may strongly influence toxicity, *e.g.* cinnamic acid.

We might discuss an interesting series of results on phenols published by Dr. Hunkel and the writer. Here H ions seem not to be much of a factor since phenylic compounds dissociate very little as a rule. Here toxicity is associated rather with the presence of certain radicles in the molecule and sometimes varies with the position of these radicles in the molecule.

Conclusion

In reviewing the results just indicated one is led to ask again,—What lies back of these properties of ions and molecules that makes them harmful? We have tried to parallel these properties with various physical and chemical attributes, and, barring the rather bad fit seen in the solution tension series, there seems to be little chemical and physical order in the series of toxic equivalents. Looking at it through the spectacles of physiology, instead of those of physics and chemistry, we see plants damaged by water solutions of a good many kinds of things; some of these things exist only in the chemical laboratory, some are found only locked up in the very slowly-soluble minerals that make up the earth's crust, others are abundant and relatively easily soluble. Hence the vegetable world has no acquaintance with many of these compounds that we have brought to them. Plants and animals have evolved through the ages in more or less intimate contact with certain materials in their environment that have served them as energy carriers or solvents, or catalyzers or as constituents used in their structures. What substances have green plants thus either used or become accustomed to? Obviously those that are the most widespread

in their distribution and that go fairly readily into solution. Some substances have a wide distribution, but are so difficultly soluble that the plant world knows them only as ions in very dilute solutions. Such is the case with Fe, Al, Cu, Zu and other heavier metals.

The more abundant and soluble constituents of the earth are indicated in analyses of soil solutions, river and spring waters and in that of the ocean. For the rainy portions of the country the solutions in which plants meet their chemical environment show characteristics easily seen in the three analyses that follow. One sample is from the St. Lawrence River at Montreal, the second is from the Potomac near Washington, D. C., the third from the Mississippi River at New Orleans.

TABLE VII

Dissolved Content of River Waters	St. Lawrence	Potomac	Mississippi
CO_2	44.43	44.37	34.98
SO_4	11.17	7.68	15.37
Cl	2.41	4.44	6.21
NO_3	—	—	1.60
Ca	20.67	27.40	20.50
Mg	6.44	4.08	5.38
Na	4.87	2.83	8.33
K	—	.55	—
SiO_2	10.01	4.56	7.05
Al_2O_3	—	4.09	0.45
Fe_2O_3	—	—	0.13
Total salinity	100	100	100
Parts per million	148	115	166

The constituents present in sufficient quantities to be listed here by the analysts are CO_2, SO_4, Cl, NO_3, Ca, Mg, Na K and SiO_2, Al_2O_3, and Fe_2O_3. Those making up more than 4 per cent of the total ash are CO_2, SO_4, sometimes Cl, Ca, Mg and sometimes Na and SiO_2. Sodium usually predominates over K in these natural waters in a ratio from 2 to 1 up to 4 to 1.

I think that we will agree that in this list of the more abundant ions appear those used by plants and all of those listed among the more toxic ions are absent from the table. One element is both absent and almost non-toxic to lupines. That is the cation, *Strontium*. It is neither abundant in the earth's crust, nor is it very soluble, and, if the non-accommodation theory of toxicity here advanced were to apply, it should be toxic. Such is not the case, Sr being as free from injurious effect here as Ca itself. The question then arises, can it be that these two ions are so much alike that the lupines do not distinguish them? It would be very interesting to know whether Sr could replace Ca with lupines in all physiological processes.

The waters of the ocean into which the dissolved materials of the land have been flowing for æons also furnish an indicator of the water soluble materials in the earth's crust. The Challenger analyses of many samples of Atlantic Ocean water show: Cl 55, 18 per cent of total solids, Br. 0, 179; SO_4, 7.91; CO_2 0, 213; Na 30, 26; K 1.11; Ca 1.24; Mg 3.89. Average total salt content 3.63 per cent.

We must now ask, perhaps rather late in the discussion, whether the toxic equivalents found with lupine plants apply to other species as well, in short, whether the plant protoplasm that faces the exterior world of soluble things is all the same substance. If it is, it might with reason be expected to produce a like result when exposed in any of its many manifestations to a very definite chemical and physical environment. If toxic equivalents differ distinctly and decisively for different plants however we shall find it necessary, if the accommodation hypothesis is to be applied to the facts in the case, to look for different past chemical histories for plants giving different reactions. Fortunately earlier studies have shown that two different types of plants have been detected differing in their relations to the soil. One type is seen on sandy lands that have a distinctly acid reaction and a dilute soil solution; the other demands a rich soil, a reaction nearer neutrality and requires more Ca.

Lupinus albus, cowpeas and field corn belong to the former

type, while garden peas and practically all garden crops belong to the latter type. The toxic equivalents of several salts for plants of each type have been worked out. Miss Mary A. Russell obtained the results with squashes.

TABLE VIII

Toxic Equivalents of Garden Peas and of Squashes

Substances	Garden Peas	Squashes
$Ca(NO_3)_2$	5	4
CaCl	4	4
$Sr(NO_3)_2$	10	416
$Sr(Cl_2)_2$	4	63
$Mg(ClO_3)_2$	100	250
$MgCl_2$	625	1,666
$Ba(NO_3)_2$	3,125	50,000
$BaCl_2$	3,333	2,500
$BeSO_4$	6,250	27,500
$HgCl_2$	12,910	500,000
$ZnCl_2$	19,230	25,000
$CdCl_2$	133,333	166,666

From the toxic equivalents here shown, it appears that all types of plant protoplasm do not react in the same way to like chemical substances. It is clear that the squashes are injured by smaller concentrations than are the lupines, and are, therefore, made up of a somewhat different protoplasm, chemically or physically speaking, than the lupines. *Lupinus albus* is a plant of south and southeastern Europe where it is found, as Theophrastus said, at home in poor sandy lands and seems to be half wild. The squashes and melons are associated with richer lands, the watermelon with the strongly concentrated soils of the Egyptian region, colocynth apples with the northern edge of the Sahara desert, the Rocky Ford musk melons with our own Colorado half-alkaline lands and the melon region in the likewise half-alkaline Imperial Valley, California. One has but to recall the fertilizer requirements of our gardens in order to make these cucurbits succeed if he wants evidence concerning their chemical tendencies.

If the line of argument just advanced is justified, we may not satisfy the desires of those who may wish to see all life reactions definitely assignable to known principles of chemical

and physical action, but we have found a biological principle that brings the great body of our evidence into general harmony. While to refer toxicity to the lack of accommodation by plants to chemical factors in their environment does not deal with the mechanism of their response to that environment, it seems to me to bring us nearer to a fundamental synthesis that may itself in time shed light on that mechanism. At present, the answer seems to be stated in terms of biology rather than in those of chemistry and physics.

THE WAVE PROPERTIES OF ELECTRONS

By C. J. DAVISSON

(*Read April 24, 1930*)

IN AN essay published in a recent issue of the *Proceedings* [1] our president has explained to us the nature of thought, and has pointed out its limitations. The circumstance which prompted Dr. Dercum to undertake the exposition of these interesting matters is the difficulty experienced in physics at the present time in forming tangible conceptions of certain processes and certain relationships which have been discovered since the beginning of this century—in particular, the difficulty in forming any mental picture of the so-called quantum processes or of visualizing electrons which behave in some circumstances as particles and in others as waves.

If I follow Dr. Dercum, these difficulties in comprehension arise from limitations imposed upon our thinking processes by the nature of our neural protoplasm. If we were equipped with a better kind of protoplasm, one more completely responsive to stimulations by our environment, and capable of a more varied reaction to these stimulations, our comprehension of our environment would be, or at any rate could be, more complete. Things which are incomprehensible to us with our present equipment would in these imagined conditions present fewer difficulties. An individual who has been blind and deaf since birth is capable necessarily of a less complete appreciation and comprehension of his environment than one with normal sight and hearing. And yet the individual with normal sight and hearing is, as we know, blind and deaf to great ranges of light and sound frequencies. It is conceivable even that other forms of stimulation exist in his environment for which he has evolved no receptors whatever.

[1] Francis X. Dercum, "On the Nature of Thought and Its Limitations," *Proc. Am. Phil. Soc.*, LXVIII, 4, 275 (1929).

His conception of his environment and of the processes going on within it is, therefore, imperfect and incomplete and must forever remain so. This, as I understand it, is Dr. Dercum's thesis, and it is, I think, a comforting one as it offers us a legitimate excuse for giving our neural protoplasm a much needed rest. If elements in our environment are, in the nature of the case, incomprehensible to us, it is certainly foolish of us to waste time trying to comprehend them. The difficulty in pursuing this policy is no doubt that we have no test for distinguishing, a priori, the comprehensible from the incomprehensible.

It is not my purpose to discuss this fascinating subject, but to explain to you, as well as I can, one of the circumstances to which it owes its present interest: namely, the duality of apparently irreconcilable wave and corpuscular properties which characterizes electrons. This matter has not, I think, been presented previously to the Society. Before speaking of the newly discovered wave properties of electrons, I shall remind you briefly of some of the compelling reasons we have for regarding electrons as particles. It is important to do this in order that you may appreciate more fully the difficulty involved in regarding them at the same time as waves.

It was discovered more than thirty years ago that the many varied and often beautiful phenomena which are observed in highly exhausted electrical discharge tubes—Geissler tubes, Crookes tubes, Roentgen ray tubes, and the like—are due primarily to a radiation proceeding from the cathodes of these devices. It was revealed in experiments made by J. J. Thomson in England and by Wiechert in Germany in the closing years of the last century that beams of this radiation are deflected in electric and magnetic fields, in just the manner in which we should expect them to be deflected if the radiation were a stream of swiftly moving negatively charged particles. It was found possible in fact to calculate from measurements of these deflections and other data the velocities of these hypothetical particles, and also the ratio of their electrical charge to their mass. The value found for this ratio was much

greater than the largest displayed by any kind of electrolytic ion, and from this it was inferred that the particles are much lighter than the lightest atoms. This evidence of the existence of a subatomic particle of definite charge and mass was readily accepted not only because the evidence was in itself convincing but also because the idea was not a new one. The existence of an ultimate unit of electric charge had already been inferred from Faraday's Laws of Electrolysis, and the word "electron" had already been coined to designate this atom of electricity. Also, Lorentz, in attempting to explain the then recently discovered Zeeman effect, had formulated a partially successful theory in which it was assumed that particles of definite charge and mass exist within the atom. The value which had to be assigned to the charge-to-mass ratio of these, in order to obtain agreement of his theory with Zeeman's observations, was the same as that found by Thomson and Wiechert in their more direct experiments.

During twenty-five years of intensive experimentation which followed upon the work of Thomson and Wiechert, this conception of the electron as a subatomic negatively charged particle was repeatedly justified and confirmed by experiments of the most diversified kinds. Electrons were found to be a universal constituent of matter. They could be abstracted from any kind of matter in a variety of ways. They could be vaporized from matter by heating; they streamed forth under the solicitation of light and x-rays; they were ejected spontaneously by radioactive materials. Measurements were made of their charge, most precisely in the famous oil drop experiments of Millikan. By combining this result with the most reliable determinations of the charge to mass ratio, one could write down a value for the mass of the electron correct probably to within a few parts in a thousand. Estimates could be made of its linear dimensions on the assumption that its mass was entirely electromagnetic. If any doubt had existed regarding the corpuscular nature of electrons, it must have been dispelled by the beautiful experiments of C. T. R. Wilson in which the tracks pursued by individual

electrons in traversing a gas are rendered visible. The discreteness of electrons is further attested by the fluctuations which are observed in the current flowing from a heated filament; these are of just the character and magnitude to be expected for the random emission of charges of the known magnitude of electrons.

The corpuscular nature of electronic radiations had been verified in what seemed every conceivable way. The conception seemed adequate and sufficient for all demands which might be made upon it. An elaborate theory based upon this conception of the electron had been built up to explain the optical and electrical properties of matter—and this conception was fundamental also to the famous theory of the atom devised by Bohr. It cannot be said, however, that this electron theory of matter was uniformly successful in all of its ramifications. It was, in fact, the deficiencies of this theory together with certain new conceptions from the field of optics which led Louis de Broglie to suggest about five years ago that the conception of the electron as a particle might in certain circumstances be found inadequate. The circumstances contemplated were those in which the system under consideration is one of atomic dimensions. It was de Broglie's idea that in cases of this kind certain waves which he conceived of as associated with electrons might be expected to manifest themselves. The conception grew out of the reverse situation in optics in which light had come to be recognized as having corpuscular as well as wave properties, out of the mysterious correlation of frequency and energy which we meet with in quantum phenomena, and out of the correlation of mass and energy which appears in the theory of relativity. These were the antecedents of de Broglie's idea, and yet in the last analysis the idea was arrived at by a brilliant leap of the imagination.

It has been immensely fruitful. It has led to a new and remarkably successful conception of the atom from which the corpuscular electron as an essential feature has altogether disappeared. The planetary system of electrons conceived by

Bohr is replaced by a medium continuous though inhomogeneous, capable of natural vibrations. The fact that these vibrations take place in general in a space of more dimensions than three, and that we have as yet no idea what it is that vibrates, makes visualization of atomic processes a discouraging enterprise, and yet this is less disturbing to the theoretical physicist than might be supposed. He has outgrown the ambition of Lord Kelvin; he no longer tries to devise a mechanical model of every phenomenon. It has been discovered in fact, that a certain æsthetic pleasure is derived from dealing in calculations with symbols which evoke no mental pictures whatever.

De Broglie's idea has been invaluable not only as the basis of a new theory of the atom, but also as the basis of an entirely new theory of mechanics. And in these developments de Broglie has been himself a leader. In its turn the new mechanical theory has suggested experiments by which the wave-like aspects of electrons might be demonstrated. Many of these experiments have now been made; it is of a few of them that I wish particularly to speak. The simplest of all is the experiment by which it is demonstrated that electrons are regularly or "specularly" reflected from the surface of a crystal. We find when a stream of electrons is directed against the face of a crystal that some of the incident particles return from it without loss of energy, and that most of these recede from the crystal face in the direction of regular reflection. The observation is illustrated in Fig. 1. The incident electrons approach the crystal in this particular case along a direction which makes an angle of 38 degrees with the normal to its surface. The curve on the right indicates the way in which the electrons scattered without loss of energy are distributed in direction; most of them depart in a direction lying in the plane of incidence and making with the normal to the crystal face the same angle as the incident beam. There is a strong and well defined beam of regularly reflected electrons. This phenomenon cannot be explained in terms of atoms and electrons as previously conceived.

Picture the crystal built up of atoms, each of them enormous in size compared to an electron and each of them comprising a nucleus surrounded by a large number of electrons rotating in closed orbits. Imagine now an electron plunging into this galaxy of planetary systems. It is obviously a comet. The simplest event which may ensue will be a cometwise deflection of the electron in the field of some atom into which it happens to strike, and then a speeding away of the electron from the crystal without loss of energy. The direction taken by the departing electron will be determined by a number of circumstances, one of which will be the distance

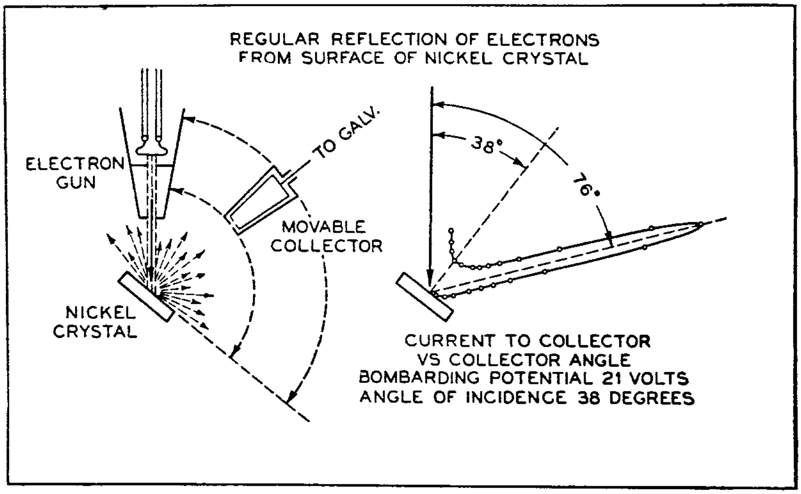

FIG. 1

from its line of approach to the nucleus of the atom responsible for its deflection. This distance will be different for different electrons—and as a consequence electrons will be scattered more or less uniformly in all directions. This is the picture of electrons scattering in terms of Bohr atoms and purely corpuscular electrons, and it is quite inadequate to explain the strong beam of electrons which is observed leaving the crystal in the direction of regular reflection. This is a direction related to the plane of the crystal surface. Three atoms at least are required to fix this plane which means that the

incident electron has its direction of departure determined not by one atom alone but by three atoms at least. On the older view we should have to suppose that the incident electron in some way takes account of the positions of not fewer than three atoms, and from characteristics of the reflection which I shall mention later we should have to suppose the actual number to be much greater—fifty or a hundred at least.

If on the other hand we regard the incident beam as a beam of waves instead of as a stream of particles, the regular reflection is readily explained; each wavefront of the beam comes in contact with all the atoms, and the regular reflection results, as in the case of x-rays, from constructive interference among the coherent secondary wave trains proceeding from the regularly arranged atoms of the crystal. Moreover, this view of the phenomenon enables us to understand the characteristics of the reflection to which I have already alluded—namely, the way in which the intensity of the reflected beam varies with the speed of the electrons and their angle of incidence.

The regular reflection of electrons from crystal surfaces is sufficient to establish the convenience of the conception that electrons are waves. The usefulness of the conception is not, however, limited to this particular phenomenon. There are many ways of demonstrating that x-rays are waves—or perhaps we should say, of demonstrating the convenience of the conception that x-rays are waves. Nearly all of these demonstrations have now been made also with electrons. These include the analogues of the Laue diffraction of heterogeneous waves by a single crystal, of the Hull, Debye-Scherrer diffraction of monochromatic waves by crystal aggregates, and of the diffraction of monochromatic waves by ruled gratings and narrow slits. The data of these experiments are available for the calculation of electron wavelengths, and these have the values predicted by de Broglie—a stream of electrons, each of momentum p, behaves in these diffraction experiments as a beam of waves of wavelength inversely proportional to p, the factor of proportionality being the Planckian constant h.

To further illustrate these newly discovered properties of electrons I shall show you lantern slides of two very beautiful diffraction patterns produced recently by Drs. Eisenhut and Kaupp in the laboratory of the I. G. Farbenindustrie at Ludwigshafen in Germany. The first of these was obtained by directing a beam of high speed electrons through a thin film of silver, and intercepting the transmitted electrons by a photographic plate. The film is an aggregate of tiny crystals of random orientation and the pattern of rings which appears on the plate (Fig. 2) is just the pattern which is calculated

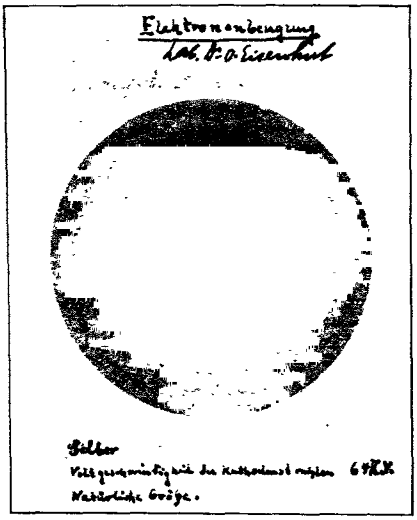

FIG. 2

from the crystal structure of silver and the assumption that the incident beam is a beam of monochromatic waves. The wavelength of the waves may be calculated from the data of the experiment and compared with the theoretical wavelength calculated from the momentum of the electrons by means of de Broglie's formula. The agreement, as has been found in all such cases, is within the limits of accuracy of the measurement. Diffraction patterns of this kind were first produced with electrons by G. P. Thomson of the University of Aberdeen.

The second pattern I shall show you is by the same investigators, and is for electrons of the same speed. The difference is that the diffracting material is, in this case, a thin lamina of mica. (Fig. 3.) Patterns of this type were pro-

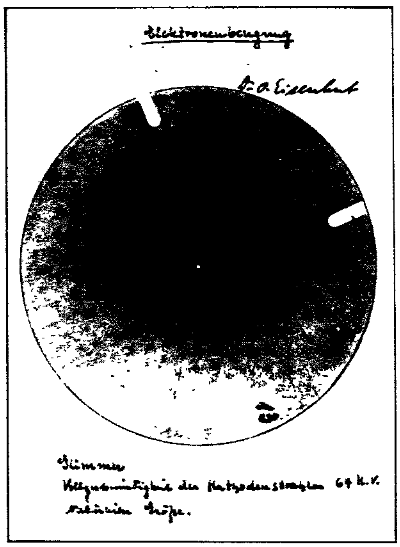

FIG. 3

duced first by Kikuchi in Japan, and for some time there was no satisfactory explanation of them. The diffracting system is a single crystal; the electrons are homogeneous in speed, the waves are monochromatic. Now it is well known to those familiar with the theory of x-ray diffraction that in general no pattern is produced when a beam of monochromatic waves traverses a single stationary crystal. One or two diffraction beams may possibly appear, but if so the event will be fortuitous; in general no beams will be observed other than the directly transmitted primary beam. Kikuchi should have known better than to make this experiment, but he did make it, and this pattern by Drs. Eisenhut and Kaupp is a beautiful example of the result he obtained. What appears to be the correct explanation of the production of this pattern has been given us recently by W. L. Bragg in England and

independently by S. B. Hendricks in Washington. Bragg and Hendricks assume that the mica crystal is to a certain extent a crystal aggregate—not an aggregate of crystals oriented at random as in the case of the film of silver, but an aggregate of tiny flakes which fail to form a perfect crystal only by being tilted slightly this way and that. This assumption together with the excessively short wavelength of the high speed electrons employed in these experiments is sufficient to explain the production of this pattern. It turns out to be, to a close approximation, the pattern which would be produced if the diffracting systems were a single layer of molecules instead of some hundreds of layers as it actually is. These patterns also are available for calculating electron wavelengths and again the agreement with the de Broglie formula is as nearly perfect as can be expected.

These three phenomena which I have described, the regular reflection of electrons from a crystal surface, the diffraction of electrons by an aggregate of small crystals of silver, and the diffraction by mica, illustrate the circumstances in which it is convenient to regard electrons as waves rather than as particles. Whether or not it is possible to achieve a unified conception of electrons in which these newly discovered wave properties appear consistent with their longer known corpuscular properties, or whether such an achievement is beyond the limits of thought is a question which does not worry the experimental physicist a great deal. It used to be said that a physicist regards light as a wave phenomenon on Mondays, Wednesdays, and Fridays, and as a corpuscular phenomenon on the other days of the week. This statement must now be extended to include electrons, and modified, I think, to state that he regards light and electrons as both waves and particles on all days of the week. And it might be added that familiarity with this idea is dulling his sense to its paradoxical nature.

Bell Telephone Laboratories, Inc.,
New York, N. Y.
April 12, 1930

SYMPOSIUM ON THE OUTLOOK FOR HIGHER EDUCATION IN THE UNITED STATES

ADDRESS

By ABRAHAM FLEXNER

(*Read April 25, 1930*)

MR. CHAIRMAN, LADIES AND GENTLEMEN:

My time is so brief and the subject so large and complex that, if I am to say anything at all, I must without ado plunge into the heart of things. Being neither a prophet nor the son of a prophet, I am in no position to predict what, if any, new agencies may in the future be invented for the purpose of promoting learning in the United States. I have therefore to confine myself to the examination of existing agencies, to asking myself what we may fairly hope from them.

Let me begin on a reassuring note. My own contact with institutions of learning covers forty-five years. When I became an undergraduate student at the Johns Hopkins University in the autumn of 1885, there were a few scholars scattered among the then existing colleges of the United States, and there was the Johns Hopkins University less than ten years old, very limited in its scope, but a genuine university in its efforts and in its ideals: the only university, in fact the only institution expressly and mainly devoted to the advancement of learning which at that time existed in this country. There has been enormous progress since that day, for there is hardly a section of the country, though the sections do vary very greatly, in which scholars and scientists of national and international repute are not engaged in advancing learning and in training eager and idealistic young men to carry on after their own careers have ended. The physical facilities in the way of libraries and laboratories and seminar rooms that

existed in 1885 were well nigh negligible. The sums of money available were relatively small. There were no institutes of research, few learned societies, and very limited opportunity to publish. In the interval, there has been a tremendous expansion in the form of laboratories, apparatus, and libraries; various institutes for research have been established; learned societies and councils have become numerous; journals have been started, the university press has come into existence, and financial resources have enormously increased, though not so greatly as appears, since the purchasing power of money is just about one-fifth of what it was in 1885. On the whole, it is, however, fair to say that there has been an immense and a genuine development in opportunity in the various fields of higher learning within the lifetime of those who compose this audience.

We are advancing knowledge to-day, roughly speaking, in five ways: (1) through individual scholars and scientists who may be called free lances in the field of learning; (2) through academies and associations of specialists; (3) through research institutions devoted to specific ends; (4) through foundations that award stipends for research; (5) through universities.

As far as we are now justified in guessing the future, what are we to say of the probable future of each of these methods of advancing learning? As to individuals, with the increase of wealth and with the increase of travel which brings men of wealth into contact with the patrons of art and science in Europe, it seems to me likely that independently of universities more men and women will devote themselves and their resources to the advancement of learning. Learning may become a hobby of the well-to-do, as it was with Darwin. A rich man has very few ways of using his money himself in America. He may have a few houses, but they will make hardly a dent upon his resources. He may have a yacht, but that will cost him relatively little and, if the 18th Amendment continues to annoy him and to restrict his enjoyments, he will be forced to lead a rather dull social life. Learning is an es-

cape. A well-to-do man may set up a chemical laboratory, as Dr. Christian Herter of New York did. He may become a great collector like the elder Morgan or Archer M. Huntington. He may become a connoisseur in one of the fine arts. He may even go to the extent of becoming an individual patron of music, of which development there are some signs in New York, Philadelphia, Chicago, and elsewhere. Things like this have happened in other cultivated countries: in Germany, where Schopenhauer, enjoying a competence, philosophized on his own, leading a quiet life in Frankfurt on the Main, the result of which was his epoch-making contribution to philosophic thought. In our own day, the brothers Warburg have established a superb library at Hamburg in which their eldest brother, lately deceased, Professor Aby Warburg, lived a quiet, retired, and productive scholarly life. There have been many men of this kind in England, whose names will spring to your minds. I should suppose it a safe guess that a country with more millionaires and perhaps more billionaires than any other country in the world will from time to time produce more and more individuals whose hobbies will be learning and science. In so far as higher learning is concerned, its prospects in America are from this standpoint increasingly good.

I mentioned, in the second place, academies and associations. They have become numerous in the United States. They perform a useful function in bringing together men interested in special lines of work, in enabling them to talk with one another, to read their papers, and to discuss their problems. This social side of the intellectual life is perhaps relatively more important in America than anywhere else, because America is so sadly devoid of the amenities which should accompany the intellectual life. We have in America little that is comparable to the common rooms of the Oxford and Cambridge colleges where scholars may gather informally for that easy-going miscellaneous talk, in the atmosphere of which ideas are born and threshed out or where distinguished foreigners can be entertained in a dignified and simple way; nor

have we any longer an equivalent to a German tavern or beer-garden where men may sit for hours sipping a mug of beer and discussing ideas and notions, real and unreal, genuine and fanciful, important and unimportant. It is, I think, not easy to exaggerate the importance of the informal social element in the promotion of science and learning. Professor A. V. Hill once suggested to me that the best way to advance learning in America would be to endow tea-rooms—tea-rooms, I suppose, because anything more stimulating is barred—literally barred. Aside from the stated gatherings of scientists and scholars at their academies and associations, there is in America at present little provision for the amenities of learning. In fact, there is a pretty common notion that amenities are at war with efficiency, a flagrant misconception. Men do their best thinking in curious ways. Science and learning are in the highest degree individualistic. The associations and academies which come together at regular intervals at definite periods to listen to papers and talk do serve a very useful purpose. I sometimes wonder, however, if they are not in danger of becoming too numerous and too populous. An academy or a society is one thing; a convention is another. We are as a nation so amiable that we do not like to shut anyone out: a society tends therefore to become unmanageably large. But the evil contains within itself a corrective; for when the thing gets too cumbrous, the young Turks secede, forming something small and definite, that will, however, either die or become too large to serve its purpose. Anyway, associations and academies through their sessions and their publications are more and more likely to encourage learning, even if they also find it increasingly difficult to limit their size and to exercise critical severity in regulating their program. This point—the difficulty of excluding persons or papers—is really a very serious one—more so in America than in Europe. If you will watch the newspapers, you will be amazed at the research which is often presented before specialist gatherings of national and international scientists. As I write, my memory recurs to international gatherings of sociologists and

psychologists last autumn—both perilous groups. Before the assembled sociologists a professor at the Unversity of Michigan read a paper entitled "Experimentation in Face-to-Face Interaction"; a professor at Northwestern University, a paper on "Family Bereavement." Before the ninth International Congress of Psychologists last September, a professor at the University of Rochester read an elaborate paper on "Common Annoyances." In the course of several years, he had compiled a list of 21,000 annoyances, but on eliminating duplicates and "spurious annoyances"—what are "spurious" annoyances?—he boiled his 21,000 down to 507, which he arranged on a scale marked from 0 to 30. "To find hair in food" is marked 26; "a dirty bed," 28; "cockroaches," 24; "to see a baldheaded man," 2. We might conclude by saying that associations are likely to advance learning in direct proportion to their lack of amiability.

I mentioned, in the third place, research institutions. We have in recent years established a good many of them. Only a few of them are as yet sufficiently rich in endowment, sufficiently strong in personnel, sufficiently broad in scope, to be really important. I should suppose that in the future we may expect existing research institutes to receive larger funds, to obtain an abler personnel and to broaden their scope. But the research institute suffers under certain very definite limitations. It is almost inevitably restricted in its field. Now ideas have strange ways of cutting across fields. So complex is the modern structure of learning, so capriciously do ideas shoot from one field into another that it is impossible to tell from what source the fruitful conception will come. The research institute has all the advantages of intense concentration of purpose. It is likely to have the disadvantages of not containing within itself a sufficiently varied personnel, of not representing a sufficiently varied group of interests and activities. The man who did most to develop the idea of the research institute was Althoff, for twenty-five years an important member of the Cultus Ministerium at Berlin. More than any other single person, Althoff was responsible for the enormous development of

German learning during the best years of the nineteenth century. He clearly perceived the danger to which research institutes were exposed, even while he was engaged in establishing them for the benefit of highly endowed individuals, who could not be used to best advantage in other institutions of learning. Althoff therefore made it a point to relate research institutes to universities. Luckily the universities were such that this relation has proved to be a fruitful one for both parties concerned. A similar situation, as we shall see, does not yet exist in the United States. Our universities have not yet reached the point where it is best for the research institute to be associated with them. The research institute has in America done best when it has been broad, well endowed, and independent. Of such research institutes—broad, well endowed, and independent—we have hardly more than can be counted on our fingers. Whether we are likely to have more no one can tell; I think we are. In this connection, I should perhaps say a word regarding the research institutes which have been set up in connection with great industries. They will undoubtedly contribute a good deal to practice and technique. To what extent they will be active and productive factors in the promotion of fundamental learning remains to be seen. It is not long since I urged upon the head of one of the greatest industrial enterprises in the United States the fact that in the long run industry stood to profit by the cultivation of disinterested research, research that might or might not help him or his competitors. His answer was characteristically American: "Research," he said, "costs money, and our money belongs to our stockholders, who do not appreciate the truth of your argument, if true it be." I suspect that this gentleman, head of one of the greatest industrial enterprises in the United States, was not himself convinced. We shall have, in the first place, to convince the heads of great industries and it will be their task to convince the directors and their stockholders. Inasmuch as money is made easily in the United States, the process of educating industrialists to the duty of subsidizing and maintaining absolutely disinter-

ested research may or may not proceed rapidly. It is, however, well within the range of possibilities.

In the provision of subsidies for research and research workers the country has made a great spurt in recent years. Several foundations devote large sums annually to the encouragement of learning either through training young men and women or through subsidizing specific enterprises. The administration of funds for these purposes is in so vast a country extremely difficult. If selection, either of persons or of topics, is to be wisely made, time, patience, knowledge, and insight are required. But it is a serious thing to encroach upon the time and patience of the experts who possess the requisite knowledge and insight. They are the very persons upon whom we rely for the increase of learning; they ought to be shielded, yet they alone can advise us well. We must therefore exact a sacrifice of them; but we must be careful to reduce the sacrifice to a minimum. When I contemplate the varied capacities in which American men of science so readily agree to serve—as members of the National Research Council, the Social Science Research Council, one or another of the Presidential Commissions, the Scientific Book of the Month Club, the Advisory Committee of this or that newly projected Institute, I confess that I feel certain misgivings. Can men discharge these responsibilities without breaking down under them? Or is it right to lend their names without incurring the full measure of responsibility? I have myself been asked to serve on committees with the assurance that no time or effort would be required. I also know a prominent scientist who was similarly reassured as to another enterprise, in behalf of which, on precisely those terms, some of the most distinguished men in America have been enlisted. This sort of thing seems to me utterly indefensible. The alternative is "hands off" or actual responsibility. American life is hectic enough without such distractions—even the life of scholars and scientists. How far can they participate in the management of councils, committees, and commissions without doing more harm to learning than they do good?

We come finally to our universities. What are they? A university ought to be an institution devoted to the conservation and advancement of knowledge and teaching at a high level; in this sense there is not a single university in the United States to-day. There are in scores of so-called, "universities," individuals—physicists, chemists, mathematicians, mediævalists, philosophers—who belong in universities—more, many more to-day than there were a generation ago; but in no university to-day in this country are there enough of them to determine the character of the institution. An American university is a miscellaneous aggregation, devoted to secondary education, college education, technical education, vocational education, professional education, and disinterested scientific and scholarly teaching and research. It runs the whole gamut of practical, social, scholarly, and scientific effort, from the close of the common school period to the most exacting research institute. Of course, it contains scholars and scientists, ranked by their peers with the ablest in the world; but nothing like adequate protection is provided for them. From the standpoint of the multiplicity of activities in which the American university is engaged, the term, "university," has lost its meaning; it cannot any longer be defined. There is not a single university in America to-day which can be said to pursue a university policy in the sense in which, with a few reservations, every university in Germany pursues such a policy. If you will look at the catalogue of any one of the great American universities, it will remind you, if you are fond of comparisons, of a metropolitan drug store. I recall the time when an American druggist was a modest but well-trained pharmacist, whose main business was the skilful preparation of a physician's prescriptions. What is a drug store to-day? There is a small counter in an inconspicuous place to which a prescription can be carried in order to be filled. There is a huge soda water fountain. There is a sandwich counter. There is a book department. There is a department for the sale of perfumery and cosmetics. There is a cigar shop. There is a rubber goods division, and there

are other divisions for the sale of what in the old days we used to call "notions." There is hardly an article from a handkerchief to an electric toaster that cannot be purchased in the ordinary American drug store. The American university is analogous. It is, as I have said, a secondary school. It is an extension school. It is a home study school. It is a mainly vocational law school. It is a mainly vocational medical school. It is a mainly vocational engineering school. It is a college group, and it is a graduate school mainly devoted to the turning out of persons whose higher degrees enable them to get higher salaries as teachers, that is, it is a teacher-training establishment. I have, as you note, in every case said "mainly," for in every one of these incongruous divisions you will find persons and opportunities that should be described in university terms; but when the whole thing is brought together, it produces a bedlam, a bedlam of subjects, a bedlam of ideas, a bedlam of personalities, a bedlam of numbers, an administrative bedlam, with the result that there are American universities which spend more on administration than some European universities spend altogether on everything which they do. American universities are not all equally open to this criticism, but they are all open to it more or less, most of them more rather than less.

I like to contrast the reckless development of the American university, eager for numbers, eager for buildings, eager for endowment, eager for new departments, eager for anything that some benevolent old gentleman will finance, with the placid course pursued by the outstanding school of medicine in the United States. When Johns Hopkins devoted his fortune of $7,000,000 to the founding of a university, which was during President Gilman's regime a genuine university, and to the founding of a hospital, he expressed the wish that the hospital should some day be closely associated with the medical faculty, to the creation of which he ventured to look forward. In the early 90's the Johns Hopkins Medical School was opened with an endowment of $400,000; but it had things that were infinitely more impor-

tant than endowment. It had, in the first place, an idea, the idea that it was the first and main concern of a medical school, engaged though it was in the training of physicians, to promote the development of medical science. The idea of study was from the beginning emphasized and strongly emphasized at the Johns Hopkins Medical School, and it has from that day to this continued to be dominant. That was a proper university conception and from that conception the School has never swerved. It had also a very small group of men, gathered from different sources: Dr. Welch, lately returned from Germany, bubbling with ideas and enthusiasm, Dr. Mall, pupil and disciple of the great Ludwig, Dr. Osler, Dr. Howell, Dr. Abel, Dr. Halsted, Dr. Kelly, two of them coming from Philadelphia, because the conditions here were—and still are—less favorable. Five or six men constituted the Johns Hopkins Medical School—they and their limited hospital and their meager endowment. What these men were not able to do went undone, because, had the institution attempted more, lacking as it did facilities, men, and money, its level would have been lowered. Against any such lowering of level for the purpose of expanding, Dr. Welch and his associates set their faces resolutely at the time, and to that attitude they have adhered ever since. A few weeks ago Dr. Welch, still at the height of his intellectual powers, celebrated his eightieth birthday. The policy, which he inaugurated in 1885, he has followed for forty-five years. The institution has never made a step forward unless it possessed the man, the facilities, and the means. Its most recent development was a department of ophthalmology. Nothing could have been more characteristic. There had been no ophthalmology in the Medical School worth mentioning from 1885 to 1925—a period of forty years. Did they want ophthalmology? Yes. Did they need it? Yes. Why did they not have it? They had neither the money nor the facilities nor the man. About 1925, an opportunity arose to obtain Dr. Wilmer as professor of ophthalmology. I well recollect my first conversation with Dr. Welch on the subject. "We should like to have

Wilmer," he said, "but it is out of the question unless we get a clinic and endowment." Three million dollars were raised, and only when this money was obtained was Dr. Wilmer invited to become professor of ophthalmology and to organize an ophthalmological clinic comparable to the best that existed in the Old World. It is a significant fact that this lame and maimed medical school with its small medical clinic and a small surgical clinic and its small out-patient clinic nevertheless has contributed more to the development of scientific medicine and of medical education than any other medical faculty in the United States. Why? The answer is obvious: because what it did, it did well. It did anatomy. Professor Mall's pupils filled important chairs throughout the country. It did pathology in the same way and with the same result. It did medicine and surgery and later pediatrics in the same way and with the same result. It is no exaggeration to say that this small and defective school is primarily responsible for the enormous development of medical science in the United States. Of course, there were distinguished men in special fields in other universities, in other medical faculties; all honor to them, for they worked amidst depressing and uncongenial surroundings; but the main impetus to the development of the medical sciences came from Baltimore. In the reorganization of old faculties and in the creation of new faculties the Johns Hopkins Medical School has always played the most important part. I cannot but believe that, if an American university had perceived its function and clung to its duty as tenaciously as Dr. Welch and his associates saw their duty and clung to their function, I should not to-day be describing American universities in the terms I have used. Meanwhile, it is interesting to report that this policy paid. The endowment of the Johns Hopkins Medical School in 1890 was, as I have said, $400,000, not counting the hospital at all. The endowment of the Johns Hopkins Medical School to-day, not counting the hospital, is upwards of $17,000,000. I commend these figures to university presidents, who have spread themselves all over the map with the result that there is not an

American university to-day, which is not clamoring for funds in every department of its activities, important and unimportant, useful and useless.

What of the future?

I began by saying that I am no prophet, and I am no son of a prophet. I have said that at the moment the word, "university," has in America no definite meaning at all. Can we retrace our steps? Or shall we have to abandon the name, "university," to the heterogeneous conglomeration of useful and useless, of important and trivial activities, now carried on under this term? I do not know. It may be that we shall have to invent something entirely different. It may be that the university will continue to be all things to all people, that in so doing it will among other things continue to furnish a more or less congenial environment and more or less adequate support to the scattered scholars and scientists who constitute its real glory and its main justification.

But if we want to be hopeful of universities as seats of learning, the universities must be small, not large. They must be organic, not inorganic. They must be homogeneous, not heterogeneous. They must be flexible and easy-going rather than efficiently mechanized. They must include in their faculties many types and not tend too strongly to one type. They must be comfortable places for the queer and the unusual and not simply comfortable places for the mediocre and commonplace. They must avoid, as they would avoid the plague, every influence that tends to subject them to standards that may be relevant in business, but are irrelevant and harmful in the field of learning. For the moment, the current is setting away from what seems to me sound, natural, ideal, and towards what seems to me unsound, unnatural, unideal. Will it change? Will universities begin to shed students, departments, and subdepartments? Will they furnish secretaries to their professors rather than to their administrative officers? Will the administrative officers become less important, less highly honored than the faculty? Will the professorial body rise in dignity and esteem? These and scores of

other relevant questions must be asked by one who wishes to predict the future of American learning. I can ask them, but I cannot answer them; and I cannot answer, them, because, as I have already urged, I am not a prophet nor the son of a prophet, but I cherish at heart no scepticism as to the advance of learning both in Europe and in America. When the conditions which I have described become intolerable, as they became intolerable in American medicine, they will, I suspect, be changed; but whether they are changed or not, human curiosity and human devotion, upon which the advancement of learning depends, will not be thwarted by any external conditions whatsoever. Discoveries will be made in the future, as they have been made in the past under all sorts of conditions, good and bad, and discoveries in one field will fertilize other fields. Learning will go forward just as its application will go forward; only let those of us who have some specific responsibility for its advancement do all we can to provide the soil in which it may fruitfully and easily and naturally grow.

THE OUTLOOK FOR HIGHER EDUCATION

By FRANK AYDELOTTE

(Read April 25, 1930)

THE most encouraging feature of the outlook for higher education in this country for the future is our saving discontent with conditions as they are at present. We are living in a period of change and experimentation. It seems as if no theory of education were so wild or so revolutionary as not to have some college or university devoted to trying it out. Inevitably most of the experiments which are being tried today will not succeed. Many have failed already, and more will do so as the years go on. But for all that we need not begrudge the time or money spent in their trial nor refuse our tribute to the courage and idealism of those who have dared to try the noble task of rebuilding our educational system nearer to their hearts desire.

He would see our education as it is must look below the surface. On top of the stream, most open to the view and most discussed, is what we call "College life"—a vast conglomeration of clubs, societies, fraternities, athletic teams, organized cheering, bizarre costumes, hazing, rushing, tap days, amateur dramatics, oratorical contests, intercollegiate debates, glee clubs, newspapers, magazines, college annuals, dances, house parties, conventions, student self-government associations, undergraduate committees to convert the voters of the future to every side of every possible question, to remodel the institutions of society and the State not forgetting the reform from top to bottom of our educational system as well. It is a life to try the soul of the stoutest undergraduate, and to him who survives it New York or the next world war need have no terrors. For a parallel we must look to California in '49 or the Klondike or the opening of the Oklahoma Territory.

Like those picturesque episodes in our national history to which I have just alluded, the college life of the present day is, in my opinion, as transitory as the stuff that dreams are made of—in this case not a wholly pleasant dream. Changes are coming so rapidly that we shall soon look back in wonder that such things should have been. Even the vast facilities which we have elaborated to enable thousands to watch athletic games played by a few, threaten, by the mere multiplication of athletic contests, to become superfluous, through the giving of the erstwhile spectators the opportunity to become players themselves.

Many are the illusions which have gone or are going by the board. Another is the illusion of numbers. College Presidents no longer speak so smugly of "healthy growth." College after college is limiting its numbers and giving more thought to the quality of its students. College endowments are coming to be measured not in the gross but as so much per student, and it is beginning to be realized that many poor and struggling institutions would be rich at one fourth their size. It is not too much to expect that reducing may become the new fashion.

And finally among all these changes, has come the movement which is most important of all, making for higher standards of academic work. The changes which are necessary in order that this movement may succeed are fundamental and far reaching. Under our present academic system any great improvement in the standard of quality is difficult to bring about. At present the ordinary Bachelor's Degree means the accumulation of 120 semester hours of academic credit. These credit hours are interchangeable. Most colleges and universities accept them at their face value, like checks on banks which are members of the Federal Reserve System. The standard of quality required is perforce the standard which can be maintained by a student of average ability. If any other standard were insisted upon, the whole scheme would fail to work.

Like the economic man this hypothetical average student

is supposed to buy his credit hours in the cheapest market and to sell them in the dearest. What he is supposed to want is the Bachelor's Degree on the easiest possible terms, and our ponderous academic machinery of cuts and exercises, tests and examinations, deans and sub-deans, has been evolved to insure that he makes at least some real payment for his degree in terms of work and memory. To raise the quality done under that system, to raise standards to the point where they mean more in terms of intellectual values is the concern of the whole academic world today.

While it is the almost universal opinion that our standards are not high enough, it is at the same time realized by everybody that the task of bringing our college work to a higher standard, of improving the quality of the faculties in all of our colleges and universities, of selecting better students, endowed with more energy and ambition, is an immense, and indeed, an impossible one. The effort to make such an improvement quickly, even throughout one college or university, is so tremendous that not many institutions are likely to succeed in raising their whole standard to any great degree in the immediate future. If we are to adhere to our policy of regimentation, if we are to insist that every step forward shall be taken by the whole army in unison, the improvement of higher education in this country is wellnigh a hopeless task.

If, however, we follow a different line of attack, if we have the patience to begin with a few students and a few members of the faculty in a few institutions, the task is far from hopeless. This is precisely what has been done in about one hundred American colleges and universities during the past ten years, and the results are already so promising as to constitute the happiest omen for the future. The essence of the new plan is the separation of the abler and more ambitious students from the main body, so far as studies are concerned, in somewhat the same way that the Pass and Honors men are differentiated in the English Universities. This makes it possible to give to the best a discipline equal to their powers without disturbing those who have neither the ability nor the ambition to undertake a harder course of study.

The introduction of the elective system at Harvard under President Eliot, which swept so rapidly throughout the whole country, was a recognition of the fact that different students are legitimately interested in different subjects and that the best results cannot be attained by requiring them all to take the same course. What we need to do now is to recognize that different students have also different degrees of aptitude for intellectual work and that it is unfair to hold the best and the most ambitious back to the rate of progress which can be attained by the average.

Under our traditional Course and Hour System the pace must of necessity be set by the ability of the average. Those who are not able to keep up with that pace fail and fall by the wayside. Those who are able to go faster either do much less work than is good for them, or work at tasks which they find for themselves in the vast organization of extra-curricular activities, which are a poor substitute. At their best college activities do provide training of real value. They develop initiative and executive ability, and they offer a certain amount of intellectual and artistic training, but inevitably this is haphazard, and at their worst these activities are so standardized and so mediocre as to make them a liability rather than an asset.

The evil effect of this system on our best students is not merely that it does not require them to work sufficiently hard; it is as well a positive interference with independent and thorough study. Those academic regulations which are necessary to make sure that indifferent students will do at least a modicum of work for their degree are cumbersome and hampering to their more ambitious fellows who can be counted on to do their best and whose primary needs are freedom and guidance.

It is a curious fact that American students, who have so extraordinary an amount of personal freedom, have on the intellectual side almost no freedom at all. In this respect the traditional academic system of our universities and colleges follows the method of the secondary school, and the result

is to preserve in our college and university students a youthfulness of attitude which contrasts most unfortunately with the intellectual maturity of European students of the same age.

The virtue most in request in the academic world of the United States is docility. Now docility is a convenient virtue from the point of view of the teacher, but from the point of view of the student it is subject to the law of diminishing returns. While the world will always have a place for the man who can be depended upon to do whatever he is told to do at the time he is told to do it, it has much larger rewards for the men who can be depended upon to do it without being told.

For all these reasons it seems to me that the outlook for the future improvement of our standards in higher education is dependent upon the success of this experiment which, in a certain number of our colleges and universities may almost be said to have got beyond the experimental stage, which seeks first to separate the abler and more ambitious students from the main body, and second to give to these a more severe course with a much larger measure of freedom to work and finally to measure their success by much more severe examinations at the end of this course.

All the points I have mentioned are, I think, important. No matter how high the level of ability in a given college or university, it will still remain true that the best will be far removed from the average, and any standards which would be fair for the whole group will be inadequate for the ablest.

But what these ablest need is not merely more of the same kind of thing that we give to the average. Above everything else they need more intellectual freedom; they need more scope for the development of their own intellectual initiative and independence; they need to learn to work for themselves. Only by doing this can they reach that degree of intellectual maturity which they have a right to ask and which we have a right to expect.

They need finally a more severe test of their intellectual attainments. American college examinations are not suf-

ficiently comprehensive, are not sufficiently severe. The student who takes ten examinations each year, as do nine tenths of all those in our colleges and universities, comes to treat them as stunts, as memory tests, for which he will cram up a few facts to be conveniently forgotten as soon as the test is over. They cannot be ordeals for which he will summarize and organize the whole of his knowledge of a subject, for the simple reason that they are too frequent and too fragmentary to make that possible.

I can best illustrate the general statements which I have just made by describing briefly the conduct of honors work in the college which I know best, where it has been elaborated to meet precisely these difficulties which I have been describing. At Swarthmore it is open to every student at the end of his Sophomore Year to apply for permission to spend the last two years of his college course in working for honors. If he makes such application, his case is considered by a committee of the faculty which has charge of the particular field in which he wishes to work, and he is allowed that privilege, provided his record in the first two years and his individual ability, as it is known to the committee, seem to be good enough to promise success. This is not merely a matter of his average grade. Instead, the committee takes special cognizance of the character of his first two years' work in those subjects which are most similar to the one in which he wishes to specialize during his last two years. If a student proposes to work for honors in the subject of mathematics, the committee would be much more interested in his record in that subject and those related to it than, for example, in history or modern languages. If he wishes to read for honors in French, on the other hand, the committee would be primarily interested in his literary and linguistic attainments and tolerant, perhaps, of a lower grade of work in mathematics and science.

An honors student spends his last two years in specializing, not in a single department but in a field of two or three closely related subjects. As a general rule half his work of the last two years is in one department and the other half divided

between two closely related ones. In this way economics, political science, and philosophy would constitute one field of work; or, mathematics, physics, and astronomy; or English literature, philosophy and history. There are certain choices open to the student but nothing which could be called a free elective system. Instead, the various fields of study are organized by committees of the faculty, and, except for minor choices, the students in a given group pursue the same course.

The work is presented to the student not in terms of what he must do but rather in terms of what he must know. The distinction is an important one. Our college and university instruction is thought of too much from the point of view of the teacher, and in this respect the very earnestness of the members of our staffs, their anxiety that they should teach all that they ought to teach, prevents them from placing sufficient responsibility upon the student and from making sure that he has learned all that he ought to learn.

Each student who is admitted to honors work receives at the end of his Sophomore Year a detailed statement of the ground that he is expected to cover, the main periods or topics, the set of books or particular authors if any are prescribed. His sole task is to master that field. The emphasis is upon learning rather than upon teaching. The undergraduate develops his own powers and his own grasp of his field of knowledge. Not too many questions are asked as to how he does it. The various lecture courses of the College are open to him to make such use of as he sees fit. His instruction, however, will be largely individual, and he knows that the same statement which has been given to him at the end of his Sophomore Year will be given to his examiners at the end of his Senior Year as a basis for his examination.

During the Junior and Senior Years honors instruction is carried on primarily in discussion groups of four or five students. Each of these groups takes, each week, a certain portion of their field as their task for which they do a great deal of common reading, if not the same books, at any rate on the same subjects. They read papers week by week on related

aspects of the topic under discussion. These papers are read aloud in the seminar and are vigorously discussed and criticized by the undergraduates and by their Professors. Very little use is made of textbooks or predigested manuals of instruction. Instead, the students consult the original authorities on their subject and are even left a pretty large choice as to which particular authorities they will elect to read, and as to whether they will put in their time on a thorough mastery of a single book on a given subject or in a more rapid reading of several.

Perhaps the heart of the whole program is the thorough discussion which takes place—discussion which is not limited to the seminar meeting but which is continued informally in the dormitories and even in the vacations.

At the end of two years of this type of work the students are examined, not by the Professors who have been teaching them but by Professors from other universities who are brought in for that purpose. The value of these tests by external examiners is very great. They lend an air of seriousness and reality to the examination which could hardly be attained in any other way. In addition, and more important, they make the relation between teacher and pupil a wholly different thing from the conventional one. Professor and student are allies in preparation for a test over which neither of them has any control, and the relationship thus produced is perhaps the finest single feature of honors work. Examinations of this type become inevitably tests of ability as well as attainment, as all examinations ought to be.

The method which I have described as being in force in Swarthmore would apply with various changes to a number of other colleges and universities throughout the country. In many cases the change from the conventional procedure is not so radical, but I think it could be said in almost every case that the steps being taken, if followed to their logical conclusion, will lead to a result somewhat like the one which I have described. This method of work which I have been describing is neither new nor original. Something like it was

tried at Michigan in the 90's. It was eventually given up, I have been told, not because the new method of instruction was unsuccessful from the point of view of students but rather because no secure place could be made for it in the university program. There were not adequate funds to provide for individual instruction. The work which members of the faculty did in connection with it was over and above their regular duties, and the plan was eventually given up largely because no one was interested or able to make it possible that it should be carried on. Something of the same kind of work was inaugurated at Harvard and Columbia almost simultaneously in 1911. In both universities it has existed ever since on a large or smaller scale.

I have been amazed and delighted to see how marked has been the change in the atmosphere of academic work produced by the introduction of methods of this kind even on a very small scale. The result gives me the greatest hope for the future. Our university faculties and our university student bodies contain a great many men and women who are in earnest about the intellectual life. The only thing they need is an opportunity. If they are to wait for this opportunity until we can make some ponderous change in our whole academic system, it would probably never come in the lifetime of any of us. But if they can be allowed to begin, as they are beginning in many places, with those individuals who are interested, the improvement which everyone hopes for will come before we realize it.

I should not like to say that the change which I have been discussing has been introduced or is being introduced without opposition and criticism. It has been criticized as involving too narrow specialization for undergraduates. The traditional attitude of the American college student on this question is illustrated by a remark which a business man recently made to me concerning his wife who was herself a college graduate and who developed a very strong interest in archeology: "I told her," said my friend, "that if she didn't stop working at archeology pretty soon, she would lose her amateur

standing." The remark seemed to me profoundly characteristic of the attitude of thousands of American undergraduates in studies as in sports. They are exceedingly careful to preserve their "amateur standing." Certainly honors work as conducted in this country is not narrowly specialized. It is well organized and carefully focused as any college course should be. It is a grave criticism of the Elective System that that it leads so many students to acquire only smatterings of knowledge of different subjects in a mistaken but well-intentioned search for breadth.

But the criticism and objection most frequently made is that such a method of instruction is not democratic. On the basis of some vague feeling for democracy and equality it has been assumed that we must give every individual the same kind of instruction and bring him to exactly the same level of mastery of the different subjects of study. This vague idea which has produced the regimentation of our academic work is, I think, in the first place utterly impossible of realization and, in the second place, wholly undesirable even if it could be realized. In education we must think of the individual and not of the mass. We have a truly democratic education if we give to each individual the opportunity to do his best, to develop his powers to the fullest extent. If democracy could be construed to mean leveling down the best to the mediocre standard attainable by the average, then democracy would be foredoomed to failure. In my opinion democracy does not mean that, but rather an opportunity of developing all types of excellence to the highest degree to which they can be developed, which will mean furnishing democracy with the leadership that is indispensable to its success.

A COMMENT ON COLLEGE ADMINISTRATION

By F. J. E. WOODBRIDGE

(*Read April 25, 1930*)

SIR MICHAEL SADLER, in his recent lectures at Teachers College, on the Sachs Foundation, used a phrase which keeps reverberating in my mind. He spoke repeatedly of "making a liberal education available for everybody." He pictured, with telling imagery and illustration, a world-wide social movement with such a motive energizing it. The phrase has haunted me. It is one of the familiar slogans of a democratic society. It is voiced as an article of faith rather than as a challenge to criticism. It asks to be accepted as axiomatic, as a genuine democratic ideal. It does not ask for examination to discover whether it is genuinely what it professes to be. But is it? Sir Michael's lectures have forced me to ask that question, and left me bewildered regarding the answer. My own faith has been shaken. I find the reputed axiom far more difficult to accept than once I did. Sometimes I find myself perilously near believing that a liberal education made available for everybody would be a democratic calamity.

It seems axiomatic to say that in a democratic society the good things of life should be made available for everybody. Such an axiom expresses the society's primary social aim. It presupposes, however, that any of the good things of life, when made available in that fashion, will continue to be good. The risk is run that it will not so continue. The method of making it available becomes an increasingly important consideration. Now there is no accepted definition of what a liberal education is, if we look for a definition in terms of a course of study to be pursued. Definitions are usually made in terms of a set of desired traits of character which it is hoped or expected that the course of study will

produce. Experience, however, shows that these traits of character depend so little on the course of study, and so much on other factors, that the course of study admits of a wide range of flexibility. Our colleges generally profess to offer a liberal education to their students, but a comparison of their courses of study and requirements for degrees reveals such a diversity of ways and means, that college graduates, even if they are liberally educated, do not allow that education to be defined in terms of what they know. This fact is so well established that it should be whole-heartedly recognized in every attempt at educational improvement. It makes of a liberal education the attempt to secure desired traits of character by going to school, irrespective of what is specifically taught as knowledge. To make a liberal education available for everybody becomes, therefore, in practice and in educational administration, making schools so available. It implies sending everybody to school and keeping everybody in school as long as possible. Now is this a good thing to do on democratic grounds?

That is a question which I find difficult to answer. I must admit, like everybody else, that a society made up of liberally educated people would be a very fine society indeed. I should like to live in such a society, and would gladly have everybody so live; but I have reached such perplexity of thought that I am now in need of being convinced that multiplying schools and prolonging the school age is a good means of securing the society so much desired. I want to hold fast to democratic ideals, but I must confess that I find some of them sadly shaken when I contemplate the picture of a society every member of which is expected to spend at least the first quarter of his life going to school. I begin to wonder if my conception of democracy—and that of many others—has not been simply that of aristocracy turned upside down—with privilege at the bottom instead of at the top. I can understand a prolonged education beginning in childhood, conducted in the intent of producing a leisure class or class élite—to use Sir Michael's words—but I am finding it in-

creasingly difficult to understand the segregation in school up to the age of fifteen and later, of the members of a democratic society, who are then sent out of school to make their way in a difficult world. I can understand leisure and leadership as things won, but I am finding it difficult to understand them as privileges given to the young through education. I am growing more and more disturbed by the mounting costs of education, and by the type of mind which it produces. I have spent the past twenty-five years of my life almost exclusively with college graduates, to find them increasingly less prepared for the kind of intellectual work I want them to do. I am impressed by the widespread complaints about our schools and colleges, and by the intellectual immaturity too generally exhibited both by the students and teachers. I get more and more surprised at the extravagance of the young, and the luxury and idleness in which they are often allowed to live. I observe with interest the enthusiasm for new schools with new propaganda, and the expectation of magic in the educational process. And I begin to ask myself: Is not much of all this a consequence of trying to make a liberal education available for everybody? Is not much of it the result of trying to secure for a democratic society a specific good, by trying to do for everybody what an aristocratic society tries to do for a privileged few? Is the true conception of democracy a society in which everybody is educated to be an aristocrat?

I do not propose to try to answer these questions. I do not know what the answers are. I believe, however, that questions like these should be active irritants in educational circles, and potent destroyers of complaisant peace of mind. They may serve, I think, to promote a sane perspective in matters of education. They may serve to fix attention not on making a liberal education available for everybody, but on the more searching matter of finding out what availability for everybody really means, when translated into a system of democratic education. There is a wide difference between a school for everybody and a school for a few. Superficially,

at least, it does not seem wise to convert the latter into the former. Yet that, with few exceptions, looks very much like what we have been trying to do. It looks very much as if we have been trying to give to everybody the type of education which is significant only when given to a selected few. It looks very much as if we have been trying to make available for the great number of the immature the goods from which only the mature, or those with some special interest, can derive real and lasting profit. I could give many illustrations of this, but let one suffice. Mathematics is commonly taught from the age of five to the age of eighteen or twenty. It is taught both for its usefulness and also because it is supposed to train the mind. The impressive fact is that the amount of mathematics known now by age twenty is negligible, compared with the time put on it, and could at age fifteen be acquired by any intelligent person in a single year. Making mathematics available for everybody for ten years is an enterprise which deserves attentive examination.

All that I have thus far said is by way of preface to the making of certain comments on current practices in our colleges. The preface has been intended to indicate a certain amount of disillusionment on my own part, and—if I may venture this—to promote a certain amount of disillusionment in the minds of others. Personally I very much wish that we could stop glorifying education as an opportunity. I very much wish we could stop sending children to the kinds of school we send them to, when they ought to go to work; that we could stop sending boys and girls to the kind of colleges we send them to, boys and girls who ought not to go, but who ought to be self-supporting and a relief to their parents instead of an expense of fifteen hundred dollars a year each; and that we could free more young teachers from the deadening experience of dealing with immature minds so long that their own minds become like what they minister to. I wish that we could recognize that after one has learned to read, write, and figure, the only solid justification of long schooling is disciplined learning, and if one does not want that he will be better and

happier without forced subjection to it. I know the arguments against this point of view, but I have ceased to be impressed by them. The reason why I have ceased to be impressed is the very simple fact, which is readily verified, that the expectations raised by these arguments are not fulfilled. All these are wishes. We are faced not with what one wishes were the state of affairs, but with a state of affairs which one must either work with or leave alone.

Now on the basis of my preface, I ask myself under what obligations is a college to its students. I look for an obligation which is not defined by some theory of education, nor by any glorification of education, nor by any desire to make it available for everybody. As yet I have been able to find but one obligation which meets these requirements. I could wish that this obligation did not exist, but recognizing its existence I think it must be reckoned with. It is the obligation of providing adequate pre-professional training for the professional schools. A real and a grave injustice is done to students by a college if it does not give them the opportunity to secure such training. Under existing conditions, a grave injustice is done to society also. This training need not occupy more than two years out of the four at college for those who desire it, that is, half their time. It occupies much less time—or, I might say, space—in the curriculum of the college taken as a whole. There remains, therefore, in the general course of study in a college, a major portion which is not affected by the clearly defined obligation of an opportunity for pre-professional training. Now what is to be done with this remainder?

This I have come to regard as the important question which immediately faces college administration at the present time. I have already said that the provision for pre-professional training is the only provision I can find for which a college is under obligation to its students, when obligation is defined practically and factually. Obligation may, of course, be otherwise defined. It is just because it may that I have come to regard the matter as of first-rate importance.

What a college does with the free portion—as I may now call it—of its curriculum, is clearly the determining factor in making a college a fit or an unfit place in which young people are expected to spend four years of their lives, at a time when they are passing from irresponsibility to responsibilty, and when they are supported, so largely at the expense of others, in a highly privileged position. What, then, is the obligation of the college in this regard?

In trying to answer this question I must ask for myself the privilege of appearing opinionated. My opinions however—let me say in self-defense—have not been formed on the basis of any theory about a liberal education or its value, but upon experience and observation. I am fully conscious of the egotism of this claim. It is the kind a man makes when he wants to be believed, or to appear imposing and authoritative. This is why I have prefaced my remarks with an exposition of some of the convictions which I find steadily growing in my mind. In the light of these convictions I now suggest that in the matter of the disposition of the free portion of the curriculum, a college is under a greater obligation to its faculty than it is to its students. I may put this suggestion in a different way by saying that a college will now best fulfil whatever obligation it has to its students, by using the free portion of its curriculum primarily for the benefit of its faculty. This is, however, something which I suspect very few of our colleges really do. They seem to entertain the belief that they are not free to do it. They seem to me to define their obligation, not in terms of the proficiency of their faculties, but in terms of the needs of their students. The consequence is, as I see it, that they have on their faculties far too few genuine scientists, historians, literary men, and philosophers, and far too many simple teachers of the immature. A further consequence is that a moral rather than an intellectual atmosphere pervades the college. The faculty becomes doers of good, and the students become those to whom good is done. I regard this as unhealthy, both for mind and morals. The healthy attitude of a stu-

dent toward a professor is, as I see it, that of respect for a scholar and not that of submission to a teacher. I once asked a group of undergraduates how they liked a new professor who had joined the college. This was the reply: "We don't like him, but we do respect him," I thought that an admirable beginning, and it proved so in the end. And at another college, I was once told, the students divided the faculty into two classes, those who knew their subjects, and those who were popular with the students. That seemed to me to be encouraging. But I must turn from anecdote to argument.

What do students going do college need? It is very easy to answer that they need to be educated, to be turned from young barbarians into cultivated men and women. It is easy to reduce this answer to certain particulars and say they need science, history, languages, literature, economics, philosophy, art, morals, manners, religion. If we examine the requirements for a college degree, we shall find they are quite generally defined in terms of this answer to the question of what students need. The answer impresses me as unsatisfactory. I do not question its truth, but I do question its workability. We all need those enumerated good things when we think of a full, generous, and liberal life. It is worth asking, however, how much of these things any man—even the favored man—has when he has reached the age of sixty, and how much of them, consequently, he can reasonably expect students to acquire in college, and acquire even under the most favorable conditions. How much do they acquire? This question is difficult to answer, because no standard by which the 'how much' may be measured is wholly satisfactory. There are, however, some well established facts. If the enumerated goods mean subjects studied, then the students acquire little indeed in the way of knowledge. They do not acquire, for example, a reading knowledge of French and German. Even that selected number of them who go to the graduate school to continue their studies, rarely acquire such a knowledge. They acquire very little

knowledge of history, science, literature, or philosophy. They make a very poor showing on ordinary tests of information. In subjects like mathematics, in which memory is a far less important factor, they make a still poorer showing. Indeed, an examination of the studies which have been made of these matters may easily lead one to conclude that a college course is a very ineffective instrument for the acquisition of knowledge.

That would, however, be a conclusion which should be scrutinized carefully. If the same tests which are given to the students to indicate this conclusion, were given to you and me, it is very doubtful whether our showing would be any better. It is this fact which is of importance. Teachers and the public are constantly blaming students for ignorance and ineffectiveness, and forgetting their own. Older people are expecting youth to acquire in college, in four years, what the older do not acquire in a lifetime. Is this sensible? Is not the expectation commonly entertained of what a college should do in this matter about as unreasonable as can be imagined? Is it not about time that more of us wholly gave up the notion that boys and girls, who are boys and girls, can learn much in college? Is it not time we recognize that they can learn very little, and will learn still less the more subjects they have to study? But there are exceptions, I have been told, and these exceptions are worth all the effort. The exceptions will take care of themselves. Besides, there are so many supremely brilliant exceptions who never went to college at all that a justification of the college by exceptions is well-nigh its surest condemnation.

If we try to estimate what students gain from college by their subsequent careers, we reach pretty much the same result. I have worked this out for one college since its foundation over a hundred years ago. The result is what one should expect. College has been but a small item in the shaping of the careers of the most distinguished, and looms large as a rule only when a career subsequently determined has given to the college a significant position, or when an

exceptional teacher has by his personal influence led students to follow in his train. Identically the same set of studies has produced careers as far apart as that of an insurance agent and that of a President of the United States. Such facts as these are common knowledge. The thing which often amazes me is that this common knowledge is so rarely translated into a leading principle of college administration.

I might go on in this vein to much greater length. If I did, the result would be this. The criticisms of our colleges are in the main perfectly sound and just, if we take as the standard of their soundness the principle that colleges should do for students what they do not do. On the basis of this result, my suggestion is that instead of trying to reform our colleges so that they will do what they don't do, we accept what they don't do as a basic principle of college administration. The alleged weakness and failure of our colleges may be an indication of their possible strength and effectiveness. Most college graduates remember their college days with pleasure, and often with gratitude, even if they can remember little that they learned, or have found little use for what they learned. They think that college days were a real good in their lives. When I examine the reasons they give for this opinion, I find that they usually amount to this, that their college days were a time of immediate values. The college may have been trying to prepare them for life, but it was resistance to this preparation, or neglect of it, and absorption in immediate college interests, which make those days seem choice in retrospection. "When I was in college," is the standard phrase. Why not, then, take the actual and acknowledged benefit of college as the benefit it should confer? Why try to substitute for it a benefit which it has so lamentably failed to give? Why not, then, emphasize in college curriculums immediate, as distinct from remote, importance, science instead of the need of science, philosophy instead of the need of philosophy, history instead of the need of history, religion instead of the need of religion? Why not stop preparing students

for an uncertain future, and start letting them live in a certain present? Why not stop the worship of needs and start the worship of goods? These rhetorical questions should be reduced somewhat to particulars. Our colleges are now offering a wide range of subjects, and supporting this practice by the argument that students should have the opportunity to be taught them, if their needs are to be properly met. In my experience, every addition of a new department which has not been made as a result of a special gift, has been made on that ground. Costs of instruction are mounting in a way which does not mean better salaries, but which means more teachers. Our colleges are dominated most by the idea of teaching in the interest of some future benefit than by any other idea. The evidence is, however, that they do not succeed, when success is measured by tests that can be controlled. I can see no good reason for keeping up a practice so unprofitable. I believe that any college which will now stabilize its curriculum and refuse to enlarge it unless adequate new funds are given for a new department, will have taken a very important step forward. It would take a more important step, if it would restrict its curriculum, consolidate allied departments, and so adjust the cost of maintaining the college to its income that it could provide college professorships attractive to the ablest minds. Too many students are now under tutelage to teachers who are incompetent in their subjects, no matter how good teachers they are. Too many of them are paid salaries in no way respectable, and which can command only the inexperienced. The college would take a further step forward, and to my mind the step of greatest importance, if it made its faculty conscious that it was their principal business to make their subjects important as subjects of learning, rather than as materials for education. In brief, let the college become a place of learning, and not a place where the young are sent to be educated. I am quite sure that this can be done if there is the will to do it. I am quite sure that chemistry, for example, can be so cultivated in a college that students looking at the laboratory will see a

place where science is flourishing, rather than a place where they go to be taught chemistry for their good. A place of real learning can command respect because it is respectable. A class-room is a very poor competitor. So I think that the free portion of the curriculum belongs to the faculty for their benefit and enlargement. They should not be allowed to waste their time in giving courses for the students' future welfare. They should give courses which have immediate value in that they represent active participation in the enterprise of learning.

What will then happen to the students? At the worst I believe that nothing worse will happen than happens at present. I think we need to stimulate that habit of thinking which that remark implies. If we can secure a good without causing any more evil it is wise to do it, and especially is it wise when the loss of that good involves no abatement of the evil. It does no damage to a college to improve it as an institution of learning, even if the improvement leaves students precisely as they were before. I can anticipate no loss coming to students from sitting before an historian enthusiastic on his own account about the French Revolution, as compared with sitting before an instructor who tries to teach them history of Europe since Napoleon for their profit. I can anticipate no loss at all, not even with the dullest of them. I may here confess that I hated history until I sat under Professor Morse at Amherst College, who lectured on history as if he were lecturing to historians and not to me. I must confess that I have gone to sleep in his classes. He would be regarded today as a poor teacher, but there are very few of his students who do not remember him as a man of power, from whom they really learned. No, I can not anticipate a loss. I have no doubt that the students would waste a good deal of time, but I doubt if they would waste more than at present.

The suggestion is, then, that the faculty should make the most of their subjects, irrespective of the needs of the students, and so create a tonic intellectual atmosphere which the stu-

dents may breathe. Under such a system the students could not be expected to stand an examination on what the professors were setting forth in their courses. In terms of current ways of speaking, the work would be too advanced for them. I think it ought to be. There is a student need about which the faculty may well be solicitous—the need of that challenge which comes from contact with what is definitely and recognizably superior. For the good of their minds, they need lessons far less than they need contact with superior minds. They do not need to have their opinions formed. Indeed, the attempt to form their opinions impresses me as about the worst method of dealing with them, because they have had so little of that experience of life which is necessary background for the intelligent formation of opinions. They need a vivid and compelling illustration of the way superior minds work. This need not imply that the students should be left to shift for themselves, or to flounder about with nothing definite to do. But what they are given to do should be given with respect for their capacities, and with the aim of cultivating in them habits of success as over against habits of failure. What is given them to do should be given in a shape so clearly defined that they themselves can judge whether they have succeded in doing it or not. Any boy can tell whether he can jump over a bar four feet high, but few students can tell whether they have satisfactorily done the work assigned them. To change from uncertainty to certainty in this latter matter is both possible and desirable. The matter has been sufficiently studied to afford expert guidance in working it out in detail, and to remove doubt of its feasibility. And it is difficult to doubt that it is much more desirable for students to be told so definitely what they are expected to know that they can discover for themselves whether they know it or not, than it is to cultivate in them the habit of trying to please their examiners. With the free portion of the curriculum devoted to the cultivation of learning by the faculty, there can still be devised ways and means of keeping students intellectually occupied in proportion to their gifts. These ways and means can be made of such a

character that their administration would not be burdensome, nor require a large instructorial staff.

The modifications in college administration I have suggested, imply a modification of the definition of requirements for degrees. I leave this matter with but one suggestion. The requirements should not be so defined that the student is immediately confronted with them on entering college, confronted with them as a set of prescriptions which he must shape his course to meet. They should be so defined that their specific character and limitations will emerge with his progress through college. Experience shows that while many existing requirements may stabilize the degree as an index that a certain course of study has been pursued, and certain general requirements met, they get in the way of the progress of the best students. College faculties know well enough that many students, in order to graduate, are now often forced to pursue courses from which common wisdom would relieve them if the degree requirements did not control, and the students' progress did. We ought to get over this absurdity. To be specific: a student finding he needs more mathematics for physics, ought not to be obliged to get more history instead for a degree. Here, again, and generally in the matter of degree requirements, the principle of immediate as distinct from remote benefits should operate.

I return to my preface. The more I have studied and reflected on college administration, the more I have seemed to find it dominated by two professed motives. One is that of providing a liberal education, and the other is that of preparation for life after college. It is an academic hope that the two go together. Both of these purposes seem to be objectionable, the first because it implies different social conditions than those we have actually to meet, and the second because life after college is so little a matter of what is done in college, and preparation for something so indefinite involves the sacrifice of immediate values for those which are remote and uncertain. The factual operation of these motives is to direct faculties fron the enterprise of learning to the

pedagogical task of teaching the young a course of study. With these motives in control, our colleges are trying to do the best they can. They are asked and expected to do it better. I wonder if it would not be wiser to ask and expect them to do something else. I wonder if they would not be more democratic if they did. The spectacle of a democracy trying to become aristocratically cultured by going to school, with the expectation of becoming more democratic as a result, sometimes seems to me to be amusing. The spectacle of a democracy genuinely supporting the enterprise of learning in places where youth may go to get an immediate sense of what learning is, seems to me to be alluring.

Columbia University

THE SURFACE PRECIPITATION REACTION OF LIVING CELLS

By L. V. HEILBRUNN

(*Read April 26, 1930*)

For a long time, it has been realized that the essential mysteries of life and the process of living are bound up in the properties of a material found in all living things, a material which can be examined under the microscope and which is called protoplasm. Both from the standpoint of interest and from the standpoint of human welfare, it is important that we study the properties of protoplasm to the best of our ability. For the biologist, the investigation of the true nature of the living substance is as fundamental as is the study of the atom for the physicist.

From the very first descriptions of protoplasm, there has been no lack of speculation regarding its physical properties. More recently, with the development of the science of colloid chemistry, it came to be realized that protoplasm was a colloid, and that, physically, it must be regarded as such. Many writers, therefore, sought to explain living matter in terms of such known colloids as gelatin or egg albumin, and they were quick to assume that any knowledge gained concerning gelatin or similar substances might be applied directly to protoplasm. Now such assumption was reasonable enough as long as no better or more direct information was available. But about fifteen years ago, in various laboratories, a serious effort was made to study the physical properties of the living substance directly. The work that was started then has progressed favorably, in spite of great difficulties, and in spite of the usual cloud of incorrect imaginings which generally develop in a new field of science, and we now have some very definite information regarding the physical, or if you please, the colloidal properties of protoplasm.

In my own studies, it became evident early, that protoplasm behaved physically in an absolutely unique fashion, that in many respects it was totally different from any known colloid. For example, the living substance is extraordinarily sensitive. Reagents or treatments which have little or no effect on inanimate colloids often cause living protoplasm to undergo a sudden and pronounced increase in viscosity, a coagulation, if you please to call it that. It is of course, well known that living things can be stimulated, that is to say, aroused to activity, by a great variety of physical and chemical agents. Now, in the protoplasm, such stimulation to activity is accompanied by a very sharp and easily measurable increase in viscosity; indeed the protoplasm becomes three or four times as viscous as before, or in other words, if its rate of flow could be measured, it would flow three or four times less readily.

In my own work, and in my own thinking, I was at a loss to account for this extremely peculiar behavior of the living material. No inanimate colloid was known to behave in this fashion. Could one hope for a physico-chemical explanation? Was not, perhaps, protoplasm a mysterious entity which would forever resist our attempts to pry too deeply into its mechanism? We had gone a certain distance in our physical study, only perhaps to strike a wall when we attempted to interpret the unique physical behavior of the living substance. Here was a challenge, clearly expressed. Was there a possibility of explaining the behavior of protoplasm on known physical and chemical principles? In the search for such explanation, I was led to investigate a type of reaction characteristic of living cells, a reaction which will, I hope, offer valuable clues toward our understanding of the colloid chemistry of protoplasm. The reaction which I shall describe to you occurs in all living cells. It may be best studied when it is produced by artificial means, but there is little doubt but that it also occurs naturally. And when it does occur, it is capable of producing the coagulation or rather the viscosity increases which I mentioned earlier.

At the outset, I must say that my studies in this direction are only at their beginning. I can, therefore, merely outline some of the progress I have made, and I will spare you an account of the many difficulties that now lie in my path and which are as yet unsolved.

When a living cell is torn or broken, the protoplasm streams out, and typically, if the pressure is not too great, a film forms about the exuded droplet. This has been known since the eighteenth century, for the early students of single-celled animals often observed it. But in spite of the fact that this film formation about exuded droplets of protoplasm has been known for so long a time, and is so general a phenomenon, it has scarcely been studied at all. At any rate, there has been little or no attempt to seek an explanation. The textbooks state generally that the film is due to the accumulation at a surface of substances which lower surface tension, in accordance with a principle first stated by the physicist Gibbs. Now, as a matter of fact, it is hard to see how substances could accumulate at a surface to form a film, when the surface itself appears only after the film is formed. Furthermore, it can be shown experimentally that many of the agents which alter or modify surface tension have no pronounced effect on the film formation. So that the theory which is so confidently stated in the texts is, like many theories, quite wrong, and worse than that, it has apparently retarded rather than encouraged experimentation.

As a matter of fact, it is a very simple matter to study and experiment with the process of film formation in living cells. In large cells the films are readily seen and even in small cells, methods can be developed to demonstrate them. Figures of surface precipitation reactions are shown in my recent monograph on the Colloid Chemistry of Protoplasm. An essential point to be noted is that the films are not always limited to the outer surface of the exuded droplet. If a sea-urchin egg is compressed gradually, the protoplasm flows out slowly, and only a single film is formed when the naked living substance meets the sea-water. But with greater

pressure, and a more rapid outflow, other films or vacuole-like structures appear within the body of the exudate. By varying the pressure, every gradation can be obtained from a small exudate without vacuoles to an exudate filled with tightly packed vacuoles. Now it can be shown that these vacuoles which extend through the exudate, or even through the entire mass of the cell, owe their formation to exactly the same reaction which produces the outer film at the surface. So that a study of the surface precipitation reaction is at the same time a study of the vacuolization reaction within the main mass of the exudate or throughout the cell.

What is the mechanism of film formation? I have already pointed out that it is not due to an accumulation of surface tension lowering substances. How then interpret it?

The first thing to be noted is that the film never forms in the absence of calcium. If this element is removed by washing the cells in a calcium-free solution, or better, by adding a little sodium or ammonium oxalate, then the protoplasm flows out freely with never a sign of film or vacuole. This observation has been repeatedly made on many different types of living cells. Apparently the appearance of a film about the exuded droplet of protoplasm is due to some precipitation reaction which involves calcium. Hence the term surface precipitation reaction, which I suggested two or three years ago.

I have studied the surface precipitation reaction in the cells of worms, of clams, of crabs, in single-celled animals, and in the cells of higher animals such as frogs, and in all of these cases, the reaction proceeds only in the presence of calcium.

Secondly, in some cells the reaction evidently involves certain visible granules. Thus in the sea-urchin egg (which I showed on the screen), the large red pigmented granules play an essential role. If these granules are centrifuged over to one side of the cell, than a surface precipitation reaction can only occur on this side of the cell, and never on the opposite side where pigment granules are absent. When an egg cell is

broken, the first change is a breakdown of pigment granules, and this is followed by the appearance of films and vacuoles.

The fact that calcium is essential for the surface precipitation reaction suggested that the reaction might show some similarity to the process of blood clotting, as it occurs in higher animals. For the coagulation of the blood is also dependent on the presence of calcium, and when this element is removed by the addition of oxalate, the blood flows out without clotting. There are dozens of theories to explain blood clotting, but all of them agree that in the clotting process there is a first stage which requires calcium, a stage in which calcium reacts with certain elements known as blood platelets, and a second stage in which calcium is not necessary. In the first stage of the blood clotting reaction, a peculiar substance is produced, the so-called thrombin, and this thrombin can produce clotting even in the absence of calcium.

Now, in the sea-urchin egg, it is also possible to demonstrate that the surface precipitation reaction occurs in two stages. The first stage involves an interaction between calcium and pigment granule, wherein also a thrombin-like substance is produced, and this substance can then act in the absence of calcium.

For the past two or three years, I have been interested in the closer analysis of the first stage of the surface precipitation reaction in the sea-urchin egg protoplasm. It is possible to study this protoplasmic reaction outside of the cell. The pigment granules can be taken from the egg and their behavior toward calcium can be experimentally analysed. Here then for the first time a reaction involving known visible constituents of the protoplasm can be followed outside the cell under experimental conditions. One can indeed study the effect of this that or the other substance, or of various conditions on the progress of the reaction. Some facts have already been learned, and I hope eventually to be able to offer at least a general explanation of the peculiar fact that calcium can cause the dissolution of the pigment granule. But I have scarcely time to consider facts and theories concerning the more inti-

mate mechanism of the reaction, and I shall return to more general considerations.

It is indeed a remarkable fact that protoplasm on emerging from cells undergoes a reaction so essentially similar to that which blood undergoes when it emerges from a blood vessel. In the study of blood clotting, one of the essential problems is to discover why the blood does not clot within the vessels. So too in the case of the cell. In the sea-urchin egg, the cell contains both calcium and pigment granules. Why then, in the presence of the reacting constituents, does the surface precipitation reaction not immediately take place within the cell? The answer or at least a part of the answer is that calcium is not free, but is bound chemically. It is only necessary to free calcium in the cell, and immediately the cell becomes filled with films and vacuoles which have been produced as the result of a series of reactions strictly comparable to the surface precipitation reaction, but now occurring throughout the cell. The appearance of films, vacuoles, or other precipitation products within cells is in many cases due to the type of reaction which we have been describing. And this phenomenon of a surface precipitation reaction occurring throughout the protoplasm is extremely important to the life of the cell and its process of living. There is good evidence that ultraviolet rays, electric currents, and various other stimulating agents cause within the protoplasm reactions comparable to the surface precipitation reaction. This follows both from the visible changes produced by these agents, especially when they are used to excess, and also from the fact that in some instances at least they are ineffective in the absence of calcium. Indeed, it seems probable that when living matter is stimulated, the essential change is a surface precipitation reaction within the protoplasm. And the evidence is growing that these surface precipitation reactions with their formation of new precipitation products within the protoplasm, are primarily responsible for the increases in protoplasmic viscosity which occur whenever living cells are stimulated.

We have then made some progress toward the solution of

our problem. The peculiar physical behavior of protoplasm, its unique position among colloids, is in part due to a type of reaction which can be studied and to some extent understood. It seems as though one of the most essential characteristics of the living substance is its property of immediately forming a film about itself whenever it is injured and so exposed to the outer medium. And it is apparently true that this property, which may have been the first to differentiate the living substance from the non-living, is also responsible for much of the activity of protoplasm. Perhaps, in a sense, we can say that the living substance owes its vital qualities to the fact that it was so evolved as to be able to profit from its injuries. But whether this is true or not, it can certainly be shown that the unique behavior of the protoplasmic colloid is not a mystery forever insoluble, but that it is due in some part at least to a type of reaction which can be readily studied and in large measure understood.

THE KINETICS OF BIOLUMINESCENT REACTIONS OF SHORT DURATION

By E. NEWTON HARVEY and PETER A. SNELL

(*Read April 26, 1930*)

LIGHT production by luminous animals is connected with the oxidation by oxygen in water solution of a substance, luciferin, (LH_2) in the presence of another body, luciferase (A). The luciferin is thermostable, diffuses slowly, and oxidizes to a compound, oxyluciferin, (L_0) which can be again reduced by hydrogenation to luciferin. The luciferase, destroyed by heat and colloidal in solution, plays two rôles; one that of a catalyst, accelerating the oxidation of luciferin and second, that of supplying molecules which can be excited to luminesce by picking up the energy freed by oxidizing luciferin. These reactions might be represented as follows:

$$\text{luciferin } (LH_2) + 1/2\ O_2 = \text{oxyluciferin } (L_0') + H_2O$$

$$L_0' + \text{luciferase } (A) = L_0 + A'. \quad A' = A + h\nu$$

The prime (′) designation indicates an excited molecule. The spectral energy curve for luminescence of the animal studied, *Cypridina hilgendorfii*, an ostracod crustacean, has been determined by Coblentz,[1] who found a maximum at $\lambda = 0.48\ \mu$. This would indicate that the heat of reaction and activation is greater than 59,800 cal. the $Nh\nu$ value for light of $\lambda = 0.48\ \mu$. The principal evidence for believing the mechanism of luminescence to be as stated above, is based on the chemical behavior of luciferin and the general theory of chemiluminescent reactions and has been discussed by Harvey [2] in a previous paper.

It should be emphasized that any oxidative dehydrogena-

[1] Coblentz, W. W., Bur. Standards Sci. Papers, *21*, 521, 1926.

[2] Harvey, E. N., Nat. Res. Council Bull. No. 59, 1927. pp. 50.

tion having a heat of reaction greater than 59,800 cal. will not suffice to excite luciferase to luminescence. Only the oxidation of luciferin will do this and there is the same general specificity among luciferins from different animals that characterizes biological specificity of other kinds. Since luciferase is colloidal in character, this has a possible explanation in the specific adsorption of luciferin on luciferase molecules, the energy transfer only occurring between adsorbed molecules.

It is obvious that much important data for the analysis of the luminescence mechanism can be derived from a study of the decay of luminescence intensity when solutions of luciferin and luciferase containing excess of oxygen are mixed. Amberson,[1] working in the senior authors' laboratory, studied these decay curves by allowing Cypridina light, shining through a narrow slit, to strike a photographic film wound on a revolving drum. The light intensities were then read from the film in terms of the blackening after development. The analysis of such records showed that, with the exception of the first second, when light intensity is greater than expected, the "initial flash," the decay curve is that of a unimolecular reaction, if light intensity at any time is assumed proportional to reaction velocity of oxidizing luciferin at that time. In all experiments oxygen concentration was well above that at which luminescence intensity begins to be affected, so that the oxygen concentration may be assumed to undergo no change. In fact the luciferase might be compared to that of an oxidative catalyst whose surface was saturated with a film of oxygen.

The velocity constant was about proportional to luciferase concentration. In some experiments the velocity constant was almost independent of luciferin concentration, in others it increased with decrease of luciferin concentration. The temperature coefficient is high, $Q_{10} = 2.7$ on the average, with some variation for different 10° intervals. Stirring had no effect on the slope of the decay curve.

[1] Amberson, W. R., J. Gen. Physiol., *4*, 535, 1922.

If light intensity is proportional to reaction velocity, we should expect the total amount of light to be proportional to luciferin concentration and independent of luciferase concentration. Stevens [1] found that this was only approximately true, the total light emitted being greater than expected for more dilute luciferase concentrations as well as for more dilute luciferin concentrations.

In Amberson's work the concentrations of luciferin and luciferase used were such that the reaction could be quantitatively followed for 36 to 43 seconds. In Stevens' experiments a direct photometric comparison of the luminescence intensity was made by eye with an opal screen whose illumination could be varied by known amounts. The velocity constants were less and the reaction could be followed for 2 to 3 minutes. The time for 1/2 completion of the reaction averaged 8 seconds in Amberson's and 20 seconds in Stevens' experiments. Nevertheless every experiment showed the typical unimolecular decay curve, giving a straight line (with the exception of the initial flash) when log intensity is plotted against time. The discrepancies appear in the initial flash and the velocity constants with different concentrations of luciferin.

In order to investigate the initial flash a means of recording light intensities of very short duration was necessary. The photoelectric cell,[2] with amplification, and string galvanometer proved adequate and has been used throughout this recent work. Preliminary experiments have shown that the deflection of the string is proportional to light intensity for wave lengths approximately those of Cypridina luminescence. By mixing solutions of ink with water in exactly the same way that luciferase solutions are mixed with luciferin, the time for complete mixing, including the lag in the string galvanometer, can be determined, and was found to be of the order of 0.06 to 0.10 second. The period of the string galvanometer was about .007 second under the tension and voltage used. The

[1] Stevens, K. P., J. Gen. Physiol., *10*, 850, 1927.

[2] We take great pleasure in acknowledging the assistance of the General Electric Company, through Dr. Whitney, and especially of Doctors Hulett, Marvin and Koller who gave invaluable advice in connection with the photoelectric amplification.

time for the development of luminescence is too short to analyze properly. In many of our records the luminescence has decayed to 1/100 of the initial value in about 3-1/2 seconds with a half life of .5 second, but again the decay has been logarithmic throughout the greater part of the course. This statement is only strictly true, however, if the initial flash does not last too long. We find in fact that the initial flash, *i.e.* deviation from the logarithmic decay, lasts longer the greater the ratio of total luciferin to luciferase (expressed as grams dry powdered Cypridinas per volume of water) and with one sample of Cypridina the flash first appears when this ratio is made greater than about 5. With another sample, the initial flash appeared even when the ratio is less than 5. This may merely mean that the proportion of luciferin and luciferase is different in the second sample, which was collected in another locality. We have, of course, no way of determining the absolute amount of luciferin and luciferase in different samples.

The initial flash is undoubtedly a reality. It lasts much longer than the disturbances connected with mixing the solutions and is the expression of a much greater velocity than corresponds to the subsequent decay. If not marked, *i.e.* if of short duration, the log intensity of the flash plotted against time appears to be a straight line, but if the ratio of total luciferin to luciferase is very large (10 to 20) the slope of log initial flash against time is curved and merges into the subsequent reaction whose log intensity against time is also not a straight line, but concave to the time axis. These curves are not such as to indicate a bimolecular process. They are similar to conditions observed in enzyme reactions in general where additional factors, such as percentage of catalyst covered by substrate come into play, when substrate concentration is high compared to that of enzyme.

Accordingly we have turned to conditions where ratio of luciferin to luciferase is small and the plot of log intensity against time gives a straight line. The anomaly under these conditions is that different initial luciferin concentrations give

different slopes with the same concentration of luciferase. We have found that this slope is a function of the *total luciferin + oxyluciferin concentration*, L. If total luciferin + oxyluciferin is constant, the slope will be constant with wide variation in the proportions of luciferin and oxyluciferin, which only affect the intercept on the intensity axis. The less the luciferin concentration the lower the intercept, *i.e.* the less will be the luminescence intensity.

Plotting slope against total luciferin + oxyluciferin concentration (L), we obtain a curve of such a nature that the slope is inversely proportional to the $\sqrt{L}$ over all except the range with high ratio of L to luciferase, that is slope $= k \frac{1}{L^{1/2}} \cdot$ This immediately suggests that the slope measures the adsorption of luciferin and oxyluciferin on luciferase. In addition both the luciferin and oxyluciferin must follow the *same* adsorption isotherm. Once adsorbed on luciferase, we have the luminescence intensity proportional to luciferin concentration and falling off logarithmically according to the unimolecular equation. We must assume that only the adsorbed luciferin molecules are activated and can oxidize with luminescence. The velocity constant is a measure of the per cent of change in unit time, so that we may select the unit of time as that when half the adsorbed molecules have reacted. If 100 molecules are present and $\sqrt{100} = 10$ are adsorbed, 5 will react in 1 unit of time and the oxidation will be 5/100 or 2.5/50. With half the concentration, 50 molecules, $\sqrt{50} = 7 +$ will be adsorbed and 3.5 molecules will react in unit time or 3.5/50, a higher velocity. All the luciferin will finally react since we must consider that both luciferin and oxyluciferin are continually stepping on and off the luciferase surface. At the end of the reaction there are still 10 molecules adsorbed from the solution with 100 molecules present and 7 molecules adsorbed from the solution with 50 molecules present, but *they are all oxyluciferin molecules.* The luminescence has ceased. The velocity constant is greater with more dilute L for the same reason that the velocity constant is greater the higher the

temperature, because of the greater proportion of activated molecules, activated in this case by a change in the adsorption equilibrium. Here we have a rather unique case of the same adsorption equilibrium of reactant and resultant giving us always the unimolecular curve, but a velocity constant which varies with the concentration of L and in fact measure the adsorption of L on the catalytic surface.

FROM THE PHYSIOLOGICAL LABORATORY,
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THE GEOGRAPHY OF AMERICAN PEPPERS

By WILLIAM TRELEASE

(Read April 25, 1930)

THE following pages present summarily some high points and certain conclusions derived from an almost uninterrupted study during the past ten years of the North American representatives of the Piperaceae, a family of plants marked to an unusual degree by paucity of generic differentiation but at the same time by an enormous splitting into specific forms—vastly different when extremes are compared yet interblending to an almost hopeless degree when those most closely related are contrasted, but very instructive when intermediate assemblies are compared. In connection with this study it has been necessary to familiarize myself in a general way with the principal known collections from South America; and, in the study of some important collections from Hawaii and New Guinea, to become acquainted to a lesser extent with the Old World representatives of the family.

The facts that I wish to point out are most readily presented by a series of tabulations. In them, local representation does not add up into the total for any group because any but localized species or groups have been entered in more than one column; a few rectifications of earlier indications have been made; and the figures now given—even for North America—are only approximate, but except in very small groups this approximation is so close that its deviations from a final conclusion do not lessen the actual value for this purpose.

The first collective account of Piperaceae was published in 1753 by Linnaeus who had been able to glean from herbaria and publications seventeen species, all of which he placed under the genus *Piper* though several of them obviously do not belong to his Diandria-Trigynia, under which he keyed and placed the genus. Conformed to my present views, these

fall into genera that are geographically distributed as shown in Table 1.

TABLE 1

Piperaceae of Linnaeus (1753)	Old World	North America	West Indies	South America
Piper				
Eupiper.	5	—	—	—
Enckea..............	—	—	1	—
Artanthe............	—	—	1	—
Pothomorphe...........	—	—	2	—
Ottonia................	—	—	—	1
Peperomia.............	—	—	7	—

As in other groups of plants, the summation of the world flora as understood by Linnaeus led to a rapid increase in knowledge of its components which from that time forward has grown enormously through the exploration of regions not previously explored or concerning which information was most casual, so that the ensuing quarter-century very greatly enlarged and otherwise somewhat changed knowledge of the Piperaceae—as of other plant families.

Among significant contributors to this early advance were Swartz in Jamaica; Ruiz and Pavon in Peru, who differentiated *Peperomia*—a genus to which nearly half of the Pipers of Linneaus belong; Humboldt and Bonpland, whose South American and Mexican collections were elaborated by Kunth; and Martens and Galeotti as well as Schiede and Deppe, who collected in Mexico.

Though the efforts to keep a synopsis of the world flora up to date and its nomenclature disentangled had led to summations by Vahl, Dietrich and Steudel, the first specifically monographic study of the Piperaceae was published almost a century after the "Species" of Linneaus, by the Dutch botanist Miquel. His conclusions have not been followed generally; but this really critical botanist, relying largely on admittedly significant flower and fruit characters, felt impelled to divide the Linnaean genus *Piper* far beyond his predecessors

or contemporaries. For comparability with later tabulations, his genera are here grouped under the smaller number that for the moment are recognized as tenable; but it must be admitted that future monographers are not unlikely even to surpass him in the number of genera that they recognize as constituting the family (Table 2).

TABLE 2

PIPERACEAE OF MIQUEL (1843-4)

	Old World	North America	West Indies	South America
Piper				
(Eupiper)	33	—	—	—
Chavica	46	—	—	—
Rhyncholepis	4	—	—	—
Cubeba	14	—	—	—
Muldera	2	—	—	—
Coccobryon	1	—	—	—
Callianira	—	1	—	—
Enckea	—	9	5	11
Peltobryon	—	—	—	5
Sphaerostachys	—	—	—	1
Artanthe	—	26	20	144
Zippelia	1	—	—	—
Ottonia	—	—	1	11
Pothomorphe	2	1	4	7
Macropiper	6	—	—	—
Peperomia				
Tildenia	—	4	(2)	7
Rhynchophorum	—	1	11	9
Micropiper	44	11	21	74
Panicularia	—	—	—	1
Phyllobryon	—	—	—	1
Acrocarpidium	—	3	7	7
Erasmia	—	1	—	—
Verhuellia	(1)	—	3	(1)

Following Miquel two decades and a half after a preliminary clearing of the way, Casimir de Candolle, the third in a line of eminent French-Swiss botanists, assumed a place as the world authority on Pipercaeae which he held until his death in 1917 and still holds through posthumous publication of his masterpiece in 1923.

Imbued with the conservatism of his generation, Monsieur de Candolle merged a large part of Miquel's genera into a few,

conceived more in the Linnaean sense; but over-reliance on the number of stamens—in minute flowers congested and correspondingly hard of conclusive dissection and subject to suppressions—led him to include in sections corresponding to Miquel's genera species questionably belonging there, and when so-placed seriously obscuring otherwise evident geographic distribution. The following table (3) epitomizes his first general treatment of the family, in the "Prodromus" of 1869.

TABLE 3

PIPERACEAE OF DE CANDOLLE'S PRODROMUS (1869)

	Old World	North America	West Indies	South America
Piper				
Eupiper	111	—	—	—
Apopiper	1	(1)	—	—
Muldera	8	—	—	—
Schizonephos	1	—	—	—
Coccobryon	2	(2)	(1)	(8)
Enckea	—	9	11	16
Steffensia [1]	—	39	19	252
Nematanthera	—	—	—	2
Carpunya	—	9	2	10
Zippelia [2]	1	—	—	—
Ottonia [2]	(1)	2	1	16
Pothomorphe [2]	1	2	3	5
Macropiper [2]	5	—	—	—
Peperomia	74	41	61	218
Verhuellia	(1)	—	3	—

[1] The genus *Artanthe* of Miquel.
[2] Included, in the Prodromus, in Piper.

Through the later years of his life, even while otherwise occupied, Monsieur de Candolle kept in hand constantly a forming analytical key to the constantly increasing species of Piperaceae, into which additions were methodically inserted. Throughout, he adhered closely to the conservatism in generic division that marked the Prodromus treatment. As published after his death, this "Piperacearum Clavis" may be summarized approximately in a general way as follows (Table 4):

TABLE 4

PIPERACEAE OF DE CANDOLLE'S CLAVIS (1923)

	Old World	North America	West Indies	South America
Piper				
Eupiper	562	—	—	—
Muldera	18	—	—	—
Coccobryon	8	(2)	(3)	(6)
Sarcostemon	1	—	—	—
Enckea	—	15	8	8
Steffensia	—	222	43	485
Nenatanthera	—	—	—	7
Carpunya	(1)	37	9	68
Zippelia [1]	1	—	—	—
Ottonia [1]	(1)	9	1	20
Pothomorphe [1]	1	3	2	4
Macropiper [1]	13	—	—	—
Peperomia	303	175	89	468
Piperanthera	—	—	1	—
Verhuellia	—	—	2	—

[1] Included, in the Clavis, in *Piper*.

My own tentative views on the composition of the family in America are approximately summarized in the following table (Table 5) in which the species recognized for North

TABLE 5

APPROXIMATE AMERICAN REPRESENTATION (1930)

	North America	West Indies	South America
Pothomorpheae			
Pothomorphe [1]	4	2	2
Manekia	—	1	—
Sarcorhachis	1	1	2
Trianaeopiper	—	—	4
Pipereae			
Lindeniopiper	1	—	—
Arctottonia	13	—	—
Ottonia	—	1	22
Piper [1]	589	143	1158
Verhuellieae			
Verhuellia	—	3	—
Peperonieae			
Peperomia [1]	410	180	950
Piperanthera	—	1	—

[1] Also represented in the Old World.

America and the West Indies have been studied in all available herbarium material—and to a limited extent in the field; while the South American species are as I understand them to be from a less critical examination of the contents of the greater herbaria of the world, supplemented by several large collections of recent years, on which little has been published yet.

The genera *Manekia*,[1] *Sarcorhachis*[2] and *Trianaeopiper*[3] have been segregated, in the course of my preliminary studies, from the tricarpellary Pothomorpheae (which also includes the Old World genus *Macropiper*), having axillary inflorescence; and the genus *Lindeniopiper*,[4] with simplified but apparently compound sympodial inflorescence, from *Piper*,—to which in its ample definition the earlier known species of the other genera, as also those of *Pothomorphe* and *Ottonia*, were referred by M. de Candolle.

The present analysis confirms a belief that a further addition must be made now to these segregations within the Pipereae because of the incongruous elements comprised in the genus (or section) *Ottonia*. Primarily this is a group of Brazilian species which reaches into nearby Paraguay and reappears in a single species in the Leeward Islands: its type is *Piper* (or *Ottonia*) *Jaborandi*, and it may have originated as an offshoot of the pinnately veined South American group of species segregated from *Piper* under the name *Artanthe* by Miquel, and considered as a section of *Piper* under the equivalent name Steffensia by M. de Candolle. Like one Old World glochidiate species constituting *Zippelia* of Miquel, this austral group of Ottonias has pedicellate flowers, in which respect it is paralleled by something like a dozen species indigenous in the Mexican region, which, however, with somewhat obscure flower differences, have palmately nerved leaves, and in general seem to represent an offshoot of *Enckea*, the other large American section of *Piper*. For these I employ the name *Arctottonia*, with the East Mexican *Piper Muelleri*

[1] Fedde, Repert. 23: 313. 1927.
[2] *Proc. Am. Phil. Soc.* 67: 47. 1928.
[3] Contr. U. S. Nat. Herb. 26: 16. 1927
[4] *Proc. Am. Phil. Soc.* 68: 53. 1929.

as its type, though in passing I must point out the fact that the larger number of forms comprised in this new segregated genus are of West Mexican occurrence and that in general aspect they parallel a similarly localized group of Enckeas, while the eastern Arctottonias compare similarly with the typical or Amalago-like Enckeas. For present purposes, this genus may be characterized concisely as follows:

ARCTOTTONIA n. gen.

Shrubs of the general characters of *Ottonia*, but palmately nerved and with a tendency to peigynous adhesion of the stamens—*Piper* § *Ottonia* Auct., as to continental North America.

Type Species: *Ottonia Muelleri* (C. DC.); *Piper Muelleri* C. DC.

TABLE 6

DISTRIBUTION IN AMERICA BY COUNTRIES

	Pothomorphe	Manekia	Sarcorhachis	Trianaeopiper	Lindeniopiper	Arctottonia	Ottonia	Piper	Verhuellia	Piperanthera	Peperomia
Florida	—	—	—	—	—	—	—	—	—	3	—
Bermuda and Bahamas	—	—	—	—	—	—	—	—	—	2	—
Caribbees	1	—	1	—	—	—	—	19	—	36	—
Antilles	2	1	—	—	—	—	—	90	3	148	1
Mexico	2	—	—	—	1	11	—	89	—	103	—
Honduras	—	—	—	—	—	1	—	41	—	7	—
Guatemala	2	—	—	—	—	1	—	59	—	71	—
Salvador	—	—	—	—	—	—	—	15	—	10	—
Nicaragua	2	—	—	—	—	—	—	30	—	17	—
Costa Rica	2	—	2	—	—	—	—	290	—	140	—
Panama	2	—	1	—	—	—	—	92	—	44	—
Leeward Islands	2	—	—	—	—	—	1	34	—	14	—
Venezuela	2	—	—	—	—	—	—	77	—	67	—
Colombia	2	—	—	4	—	—	—	267	—	171	—
Ecuador	1	—	—	—	—	—	—	120	—	165	—
Peru	1	—	—	—	—	—	—	176	—	190	—
Guianas	1	—	1?	—	—	—	—	102	—	26	—
Brazil	2	—	1	—	—	—	21	311	—	158	—
Bolivia	2	—	—	—	—	—	—	71	—	120	—
Uruguay-Paraguay	1	—	—	—	—	—	1	22	—	25	—
Argentina	—	—	—	—	—	—	—	9	—	16	—
Chile	1	—	—	—	—	—	—	3	—	11	—

The preceding table (Table 6) shows, at a glance, the great numerical abundance of forms of Piperaceae in Brazil (the largest political area in their American range), Colombia (of moderate area), and Costa Rica (one of the smallest countries). If the countries were of equal area and still individually as rich throughout in species as they now are, a very different idea of their Piperaceous relations would be presented. Such a comparison is facilitated for the two large genera by the following table (Table 7) based on the unit area of Mexico with the number of known species and the area of each county multiplied by a factor representing the approximate relation of its actual area to that of Mexico.

TABLE 7

REPRESENTATION PROPORTIONATE TO AREA

	Approximate Area	Factor for Mexico	Piper	Peperomia	Both	Ratio Piper: Peperomia
Caribbees	2,200	350	6,600	12,600	19,200	1 : 2
Leeward Islands	2,000	384	13,060	5,375	18,400	2 : 1
Costa Rica	23,000	33	9,570	4,625	14,200	2 : 1
Antilles	62,100	12	1,080	2,775	3,900	1 : 2.5
Panama	32,400	23	2,120	1,015	3,100	2 : 1
Guatemala	48,300	16	960	1,135	2,100	1 : 1.5
Ecuador	116,000	6.6	790	1,090	1,900	1 : 1.5
Salvador	13,200	58	870	500	1,400	1.5 : 1
Honduras	52,900	14	575	100	700	6 : 1
Nicaragua	49,200	15	460	260	700	2 : 1
Colombia	440,800	1.7	450	290	700	1.5 : 1
Guianas	167,600	5	500	130	600	4 : 1
Peru	709,400	1	175	200	400	1 : 1
Venezuela	398,600	1.9	150	130	300	1 : 1
Bolivia	514,200	1.4	100	150	200	1 : 1.5
Uruguay-Paraguay	169,900	5	110	125	200	1 : 1
Mexico	767,200	1	90	100	200	1 : 1
Brazil	3,275,600	.23	75	43	100	1.5 : 1
Argentina	1,153,200	.66	6	10	16	1 : 1.5
Chile	289,900	2.5	8	27	35	1 : 3

Sequenced thus according to abundance of species per unit area (Table 7), the Caribbees, Leeward Islands and Costa Rica, all of which are of South American affiliation, stand out preponderatingly as rich in Piperaceae. The

Greater Antilles, and Panama which links Costa Rica with South America, follow, but far behind; Guatemala, Salvador and Ecuador come next, though lagging still more appreciably; and once more a great drop is evident as the remaining Central and South American countries are brought into line. Mexico on the north and Paraguay-Uruguay at the South foot the list except for Brazil where the relative areal parity in species is most surprisingly small; and the Argentine grain country and arid stretches, in which this family is all but unrepresented.

Such political-area comparisons do not at all express facts in biologic terms, for only a few Piperaceae are xerophytic in the extreme sense and relatively few live at altitudes which subject them to real winter weather as it is known further North or South. This should be counted in with reference to Mexico, for example, where less than half the total area proves favorable for these plants, and for the great agricultural country, Argentina; and the enormous area of the United States obviously touches at a tangent only on the range of the family through its possession of three Peperomias of Antillean kinds, in the extreme subtropical tip of Florida. But a similar explanation is not evident for the areal paucity of species in the great tropical country Brazil with its actual abundance of species.

Ignoring again for the moment the smaller genera, which are numerically insignificant, the percentage relations of species of the great genera *Piper* and *Peperomia*, as known from time to time, are approximately shown in the following table (Table 8).

From Table 8 it appears that for the world at large, and for the Old World and America respectively, the relative number of *Piper* species exceeds that of *Peperomia* by about one-half; but with an apparent tendency toward equalization of numbers as newer materials accumulate and as specific analysis becomes more critical, a tendency possibly increased also by greater field attention given to the latter genus—very hard of preservation—as methods of drying specimens are improved.

TABLE 8

SPECIES OF PIPER AND PEPEROMIA

	Piper		Peperomia	
	Numbers	Per cent	Numbers	Per cent
Miquel (1843–4)				
Old World..........	100	70	44	30
America........	212	60	159	40
De Candolle (1869)				
Old World	123	60	74	40
America	504	60	320	40
De Candolle (1923)				
Old World.	590	66	303	34
America..	913	56	732	44
My present view				
Old World....... . .	1,000	63	600?	37
America.	1,900	55	1,500	45
Average per cent. . .		60		40

The approximate parity in specific differentiation between the larger genera when they are considered on a large scale does not hold at all closely when the analysis is carried into smaller floras as can be seen from a comparison of the ratios presented in Tables 6, 7, and 9, notably in Chile, which is almost devoid of Pipers, and in the West Indian flora on the one hand which show a marked prepondrance of Peperomias, as do the Guianas, Panama, Costa Rica, Nicaragua and the Leeward Islands, of Pipers,—the very aberrant disproportion of Honduras and British Honduras undoubtedly resulting from the present inadequate knowledge of their floras.

Perhaps the most striking fact brought out by a regional tabulating of the larger subdivisions of the large genus *Piper* (Table 9) is not only the preponderance of Artanthes over Enckeas throughout but also and even more the small fraction of Enckeas (2–6 per cent) in South American countries as compared with the Greater Antilles (13 per cent); and continental North America (13–33 per cent),—except for Costa Rica (4 per cent) and Panama (2 per cent)—the South American affinity of these countries being no less evidenced here than in

the plants themselves,—and the Leeward Islands (6 per cent), the flora of which is strikingly like that of adjacent Venezuela.

TABLE 9

	Endemic American Groups													
	Pothomorphe	Manekia	Sarcorhachis	Trianaeopiper	Lindeniopiper	Ottonia	Arctottonia	Swartzea	Callianira	Enckea	Lorentzea	Artanthe	Verhuellia	Piperanthera
Antilles....	2	1	—	—	—	—	—	10	—	12	—	68	3	1
Mexico......	2	—	—	—	1	—	11	—	1	23	—	65	—	—
Honduras es	?	—	—	—	—	—	1	—	—	6	—	75	—	—
Guatemala	2	—	—	—	—	—	1	—	—	9	—	50	—	—
Salvador..	?	—	—	—	—	—	—	—	—	5	—	10	—	—
Nicaragua.	2	—	—	—	—	—	—	—	—	4	—	26	—	—
Costa Rica.	2	—	2	—	—	—	—	—	—	14	—	276	—	—
Panama ...	2	—	1	—	—	—	—	—	—	2	—	90	—	—
Caribbees .	1	—	1	—	—	—	—	—	—	6	—	13	—	—
Leeward Islands.	2	—	—	—	—	1	—	—	—	2	—	32	—	—
Venezuela..	2	—	—	—	—	—	—	—	—	5	—	72	—	—
Colombia	2	—	—	4	—	—	—	—	—	10	—	257	—	—
Ecuador .	1	—	—	—	—	—	—	—	—	5	—	115	—	—
Peru.... .	1	—	—	—	—	—	—	—	—	8	—	168	—	—
Guianas .	1	—	1?	—	—	—	—	—	—	2	—	100	—	—
Brazil... ..	2	—	1	—	—	21	—	—	—	13	—	298	—	—
Bolivia	2	—	—	—	—	—	—	—	—	2	—	69	—	—
Uruguay-Paraguay .	1	—	—	—	—	1	—	—	—	—	1	21	—	—
Argentina...	—	—	—	—	—	—	—	—	—	—	4	5	—	—
Chile......	1	—	—	—	—	—	—	—	—	—	—	3	—	—

A notable aberration in this respect is afforded by the Caribbean Islands, the general flora of which savors of South America, but in which Enckeas reach 32 per cent, surpassing even continental North America.

It is equally evident from Table 9 that in Pipereae proper the numerically smaller segregates—whether considered as genera or as subdivisions of the one genus *Piper*—are as narrowly localized in range as they are slightly marked in taxonomic morphologic characters.

Though I am not prepared at present to carry it with equal assurance into the same detail as for *Piper*, the analysis of

American Peperomias given in Tables 10 and 11, presenting respectively a general summation of Dahlstedt's study of them in 1900,[1] and my own present approximate views for North America alone, lead to comparable conclusions along the same general line.

TABLE 10

AMERICAN PEPEROMIAS. DAHLSTEDT (1900)

	North America	West Indies	South America
Peperomia			
Acrocarpidium	3	1	3
Ogmocarpidium	1	1	1
Erasmia	3	—	1
Pleurocarpidium	—	2	3
Tildenia	22	(2)	33
Panicularia	—	—	10
Rhynchophorum	15	23	31
Sphaerocarpidium	10	25	38
Micropiper	15	19	54

TABLE 11

NORTH AMERICAN PEPEROMIAS (1930)

	North America	West Indies
Peperomia		
Acrocarpidium	4	3
Pleurocarpidium	1	1
Ogmocarpidium	3	1
Tildenia	51	—
Micropiper	76	20
Sphaerocarpidium	161	106
Rhynchophorum	61	47
Hemirhynchophorum[1]	1	0
Leptorhynchum[2]	40	—
Erasmia	5	—
Panicularia	10	—

[1] Placed under *Tildenia* by Dahlstedt.
[2] Placed under *Rhynchophorum* by Dahlstedt.

Comparison of these major groups, some of which possibly deserve generic recognition as in *Piper*, shows an evident

[1] Studien über süd-und central-amerikanische Peperomien. (K. Svensk. Vetensk.-Akad. Handlingar. 33^2. 1900.)

accord for *Peperomia* with *Piper* and its more pronounced subdivisions. Unfortunately even yet the Old World species are too little known in fruit characters to have been correspondingly classified. Miquel, who had not differentiated *Micropiper* (with a viscid pseudocupule about its fruit-base) from *Sphaerocarpidium* (without such a structure), places all of both in the former group, to which some of them belong; but the Hawaiian species, as I know them, fall into the latter group unless, indeed, a tendency to bilobation of the stigma may lead ultimately to the assemblage of a very large number of Old World species into a section as yet unproposed.

Except for *P. reflexa* in the broad sense, which may be a gerontogenous species but is represented on both sides of the ocean by an approximately equal number of similar and intergrading forms, and *P. pellucida*, a species of the New World which is widely distributed as a weed, species of *Peperomia* are endemic in each of the two great regions.

Though more than twice as many American forms are differentiated by Monsieur de Candolle (Table 4) as by Dahlstedt two generations earlier (Table 10), and the continental forms have been added to far more than the insular, it is evident that, like *Piper*, *Peperomia* may be regarded on geographic grounds as primarily indigenous to the New World and, indeed, to South America and the parts of Central America which are distinctly of austral rather than boneal alliance. This is strikingly supported by the numerical preponderance in South America of the larger subdivisions of the genus (Table 10), *Micropiper* and *Sphaerocarpidium*, also with Old World representations, and *Rhynchophorum*, *Tildenia* and *Panicularia* which like the minor groups, are exclusively American.

As with most plants, the older history of the Piperaceae is obscure; so far as conclusive evidence goes, they did not appear before late geologic time, and little that is conclusive can be said about them preceding the Quaternary period. Of five fossil relicts referred to *Piper* (1 each from Europe, Sumatra and Australia; 2 from western North America), Professor

Berry tells me that only the Sumatran specimen—*Piper antiquum* Heer—passes muster as a *Piper* in his examination. The remaining six known fossils that by any possibility represent this family—one of them a wood specimen—constitute the genus *Piperites* (3 from Java, 1 from the Gulf region of North America, and 2 from Costa Rica, with one of them also recognized as of Trinidad). These last three species are of Professor Berry's own naming, and probably offer as firm an observational foundation as can be hoped for, on which to base any direct statement concerning the time of appearance and the foliar characters of the earliest peppers. One of them, *Piperites tuscaloosensis*, is from the upper Cretaceous; the others, *P. cordatus* and *P. quinquecostata*, are from Miocene deposits. If these fossils, recorded under either genus, really represent Piperaceae they indicate an ancestral palmate nervation of the leaves (as in Enckeas and a very few Artanthes).

From both general and detailed geographic analysis, however, certain fairly clear indications of the earlier history of the Piperaceae become evident.

As I understand the subfamilies, only the Verhuellieae are exclusively restricted to the New World, in a single genus of three species; no subfamily is restricted to the Old World. All of the other subfamilies are represented in both.

Of genera, *Macropiper* is exclusively of the Old World, and the other genera (*Manekia*, *Sarcorhachis* and *Lindeniopiper*) of Pothomorpheae are exclusively American except that *Pothomorphe* possesses one Old World species—perhaps comparable with an Asiatic representative of *Agave* which otherwise is restricted to America and may have been derived in modern times from American stock.

Of the genera of Pipereae, only the monotypic *Zippelia* is exclusively of the Old World, and *Ottonia*, *Arctottonia* and *Lindeniopiper* are confined to America; *Piper*, after their removal, ranging widely within the tropics of both the Old World and the New, but in endemic subgenera.

As the Peperomieae are now understood, the monotypic

Piperanthera is restricted to the New World, *Peperomia* is largely represented also in the Old World but with more American subgenera; but no admitted genus is exclusively of the Old World.

So far as one may argue from these broad facts, the Piperaceae may be considered as of American origin, since they are at once more numerous in species and more segregated into major types in the New World than in the Old.

A similar analysis of the subgeneric groups of Piper seems corroborative of this conclusion, for although six such groups were recognized by Miquel as of the Old World, they contained, for him, only 100 species, whereas for his five admitted endemic American groups he recognized 222 species; and the latest treatment by Monsieur de Candolle, ignoring his very questionable amplification of Coccobryon, recognizes for the New World three such groups with 913 species in contrast with five Old World groups but containing only 590 species.

Considered on the basis of comparative range in America (Table 6) there is little indication as to the earlier appearance of *Piper* or of *Peperomia*, for the extension of the latter genus into subtropical Florida, the Bahamas and even Bermuda is not very significant, nor is the preponderance in species of the latter in certain South American countries, for it is offset by the West Indies, and by Mexico in the Central American region. Within the genus *Piper*, *Artanthe* (or *Steffensia*) is at once less limited in range and in numbers than the other large subdivision of the genus, *Enckea;* and for the former, South America with its Costa Rican extension has produced distinctly the larger number of differentiable forms (Table 6).

So far as such indications may be trusted, therefore, *Artanthe*, or an Artanthe-like form,—perhaps, however, palmately nerved—may be regarded as prototypic of the family, and of continental South American origin, with *Lindeniopiper* and *Ottonia* as localized South American derivatives; and *Enckea*, less abundantly represented, appears to be a North American offshoot which has ranged nearly as far as the parent stock, giving rise to Swartzea in the Antilles, to

Lorentzea in the extreme south of South America, and in Mexico to *Arcottonia*.

A geographic analysis of Piperaceae in the New World also affords comparable and equally suggestive indications as to the part of America in which the family, as now understood, appeared. It may not be entirely because of its large area within the tropics that South America possesses nearly twice as many endemic representatives of the *Steffensia* or *Artanthe* Pipers, or of endemic species of *Peperomia*, as the remainder of the New World (Tables 4 and 5); for of localized genera only one (*Trianaeopiper* of four species) is South American, while three (*Manekia* 1 species, *Piperanthera* 1 species, and *Verhuellia* 3 species) are endemic in the West Indies, and one (*Lindeniopiper* 1 species) is endemic in Mexico. *Pothomorphe*, which occurs on both sides of the Isthmus, does not afford much evidence on this point, for it has obviously endemic species in Central America. *Ottonia* and *Arctottonia* are two very distinct but similar groups, one exclusively South American and resembling *Artanthe*, while the other is of the Mexican region and resembles *Enckea* (Table 5).

No discussion of such a subject as this can fail to take account of two antithetic points in geographic distribution,—the localized or widespread occurrence of individual species.

For my present purpose I do not need to go into exhaustive detail in this direction, and must content myself with pointing out that the vast majority of species—or differentiable minor forms—are strictly localized, but that a few superspecies or groups of very similar but still differentiable forms have a much larger range, and an analysis of this parallels to a considerable extent that of the still larger groups that are regarded variously as subgenera or genera.

In *Piper*, the form represented by *P. Amalago* of Jamaica ranges as far as Enckeas are found; and that typified by *P. hirsutum* also of Jamaica has essentially the same distribution as the Artanthes. The same is true of that group of black-punctulate *Peperomia* species of which the Jamaican *P. glabella* is typical: but the mention of these Jamaican plants—

first known of their kind—does not imply that the prototypes of the species are of Antillean origin and their present regional abundance points to all of them as of continental North American origin except for the *hirsutum*-like Pipers—which abound in the Costa Rican flora. To these may be added the Pipers typified by *P. aduncun* also of Jamaica, but most developed in the Andean-Amazonian portions of South America.

Contrasting with these, as examples, may be noted the group presented by Piper *auritum* of Mexico of which closely comparable forms range continuously to the Andes; a group typified by the Brazilian *P. nobile* which is very fully represented as far north as Costa Rica; a set of forms with large peltate leaves, centering in the Isthmian region; and two styliferous groups equally South American in evident origin. Likewise in *Peperomia* a small group of acaulescent tuberous species ranges from the Peruvian Andes nearly to the northern limits of the genus in Mexico. None of these groups is represented even suggestively in the West Indies except that the *nobile* form has reached the Lesser Antilles from the South American mainland.

Such progressively localized groups or superspecies obviously fall between progressively localized genera or subgenera on the one hand and quite localized species on the other; and in some respects they afford comparable evidence as to the relative time and place of their appearance.

Though it is quite apart from my present purpose to discuss in general either an Antillean bridge between North and South America or submergences of Central America somewhere above as well as at the Isthmus, I must note that so far as the one family goes the Piperaceae point almost unquestionably to a separation of the continents between Costa Rica and Nicaragua some time after the appearance of an Artanthoid or Pothomorphoid prototype of *Piper*, apparently in middle Tertiary time.

A special interest always attaches to the West Indies in the analysis of any family of plants occurring on both of the

Americas. Many years ago Baron Eggers, a man of large field experience, pointed out that the deep gash known as the Anegada Passage, which separates the northern Greater Antilles ending in St. Thomas, from the southern Lesser Antilles or Caribbees beginning with St. Croix, marks the division between a flora with North American affinities in the former and a flora with South American affinities in the latter. What I have called the Leeward Islands, off the South American coast, obviously are fragments of the southern continent. The Piperaceae, like *Agave* and *Phoradendron* which I have analyzed in detail earlier and elsewhere, are in accord with this conclusion, and they contribute still farther to my belief that the Anegada Passage marks a moderately recent separation between the two parts of what was originally a bridge nearly or quite connecting the continents.

Apart from species, which appear to be endemically localized, and from wide-spread groups of species, or subgenera or generic-segregations, *Manekia*, *Piperanthera* and *Verhuellia*, which are the only endemic West Indian genera, are confined to the Greater Antilles. Manonotypic except for the latter, which has only three species, these do not necessarily indicate so long an isolated existence as would be the case if they were more numerous in species or individuals than is known to be the case; but it may be argued from the somewhat more extensive distribution of *Verhuellia* that the others are of distinctly later origin than it is.

Two outstanding aberrations appear as yet in a comprehensive study of the Piperaceae; the occurrence of this strictly localized and numerically very small genus, *Verhuellia*, so markedly differing in habit and floral characters from other representatives of the family as to compel its recognition as constituting a subfamily distinct from these; and the very wide-spread occurrence of the similarly small genus, *Pothomorphe*.

Two factors of prime importance are commonly recognized as entering into the formal differentiation of living things: age with attendant dispersal; and uniform or varied ecological

conditions through their range. Perhaps the first is most commonly thought of as accountable for major segregations such as genera, and the second when minor specific or varietal differences are considered. Such differences, however, may be major and minor only in an abstract way, and it is quite possible to think of localized and even recent mutability as accountable for deviations from generic as well as specific standards. This qualification has to be borne in mind when considering the significance of numerically smaller groups, and particularly when, as in this family, distinction between generic and subgeneric, as well as specific, segregates is likely to be subjective to a considerable extent.

The possibility must be considered also, therefore, that the Pipereae have evolved from *Pothomorphe*-like ancestors which were not so labile as their *Artanthe*-like offshoot; but this concession does not affect the general conclusion that the family is of American and, indeed, of South American origin.

Another direction from which the same question may be approached is that of morphological primitiveness or approach to it or deviation from it. If from this angle the general conclusions derived from a distributional study were contradicted, these would not be presented; but these conclusions are not in disaccord with such morphological study as I have made, the Pothomorpheae and Pipereae being most nearly conformed to the tricarpellary-hexandrous type which I take for that of the family, and the monocarpellary-diandrous Peperomonieae, often with whorled foliage, appearing thus to be a later or even collateral derivative. In the first two groups the ovule is invested by two ovule-coats as in the only admitted relative of this family, the Saururaceae; while in Peperomias there is only one such coat;—raising this somewhat serious question as to their direct derivation from either of the foregoing lines. The puzzling genus *Verhuellia* has not been studied in this respect, and its ovule-structure, when ascertained, may afford a convincing suggestion as to its location in the Piperaceous family tree;—not now evident.

THE UNIVERSITY OF ILLINOIS.

LENGTHENED GROWTH PERIODS AND CONTINUOUS GROWTH

By DANIEL T. MACDOUGAL

(*Read by title April 25, 1930*)

ORGANISMS inhabiting land areas, with the exception of those in a few restricted regions in the tropics, are in an environment in which the principal components undergo periodic or seasonal changes. Of the included variations those of temperature, light and water supply are the most important.

The activities of plants in growth and reproduction are carried on in ranges of intensity or concentration of the environic factors which are specific. There are special maxima and minima of temperature, light, both as to intensity and duration beyond which the plant slows down to a rate at or near zero. There is also a region in which the rate is highest. All of these points are relative, not absolute. The intensity of one fact or affects the rate of activity with respect to another in accordance with the well-known principles of limiting factors.

By reason of the alternation of favorable and unfavorable temperatures and other conditions plants, especially those of the temperate zones, are forced into an inactive condition, especially as to growth during a large proportion of each year. Thus an eastern hardwood tree such as the beech, maple or oak does not show growth of the trunk for more than 40 to 70 of the 365 days of the calendar. This periodicity is to be defined as a seasonal response, or as organographic, and not resulting from any fundamental rhythm or periodicity of the protoplasm of embryonic or meristemmatic tracts.

Favorable intensities of environic factors are indispensable to division of the cambium, as well as for enlargement and differentiation of the derived elements. A constant and ade-

quate supply of water and building material is a further condition of such activity. If the cambium sheet of a tree trunk uses carbohydrates faster than these materials are received from the leaves there must inevitably come a time when the lack of material will result in retarded growth. Then again if the continued growth of a tree through a long season be considered it is to be noted that the suite of deciduous leaves formed in the spring and which are not replaceable or renewable during the summer become torn, coated with dust or clogged with internal mineral deposits with continually lessening efficiency as photosynthetic machines so that the amount of material made available for the cambium or other embryonic or generative layers is much less in August than in May or June. Similar disintegrations and resulting diminished capacity take place in the root-systems. Trees with such organization will consequently show a seasonal activity suggestive of rhythm though the temperature and other climatic factors continue in a favorable intensity. Growth cannot be resumed until new organs for absorption or food formation are acquired.

Continuous growth may be expected only in trees of the type known as "evergreen" which retain every leaf for two or three years and which form a new set of these organs annually or at shorter intervals. Pines and some oaks are illustrative of this type although but few species of these genera are native to regions in which the climatic conditions make possible a continued growth.

The general considerations noted above may be briefly restated as follows:

1. Seasonal activity in plants may be determined by the intensities of such environic factors as temperature, water supply, or illumination, or by the deterioration of deciduous organs such as leaves.
2. Continuous growth in plants may be expected only in species carrying on a morphogenic program by which they at all times are provided with food-forming or chlorophyllose tissues and absorbing organs.

3. A supply of water and a temperature range between the minimum and maximum are also necessary for uninterrupted growth.

Scope of the Present Paper

Additional evidence is cited in support of the conclusion that the cambium layer in trees has no innate rhythm or periodicity, and experimental work which yielded information upon which a comparison of the growth of a walnut tree in its native habitat with a short season with the activities of other individuals in an equable climate in a lengthened season could be made is described.

The results also included observations upon the growth of a pine in the equable climate of its native habitat, in which the environic factors were consistently within the tonic range except for brief intervals widely separated, when the temperature fell below the point necessary for the activity of the cambium.

Modifications of members and tissues laid down under the influence of environic factors which extend the growing season are described.

The experimental work with Juglans was carried on in connection with a series of transplantations begun in 1906. The native habitat of this tree was in montane area in a wooded valley opening northward on the east flank of Mt. Lemmon in the Santa Catalina Mountains at an elevation of 8,000 feet. The frostless season had a duration of 138–153 days; precipitation during the summer amounted to 19 to 28 inches coming chiefly during July and August; the rains and snows of the winter gave a similar amount. Winter temperatures of $-9°$ C. and midsummer temperatures of 30° C. were registered. The general character of the climate is suggested by the presence of *Abies concolor*, *Robinia Neo-Mexicana*, *Acer brachypterum*, *Pinus robustum*, *Juglans major*, *and Symphoricarpus oreophilus*.

One of the limiting features of this montane habitat was the fact that the winter rains and snows were received chiefly

before April and that after this time the surface layers of soil underwent a continuous decrease in soil moisture except when watered by streams, so that by the beginning of the summer but a scanty amount of growth was possible. Activity was displayed only by plants in moist areas near streams and springs. The frostless season ended early in September.

It was from conditions described and implied that nuts of *Juglans major* were taken to be germinated at the Coastal Laboratory at Carmel, California, in 1909. The actual site on which young trees were established was on the eastern slope of an arrested dune. Some parts of it were moistened by springs and Pine No. 17, the action of which will be described below, stood in this area. The rainfall of the region averaged 18 inches, coming chiefly between December 1st and April 1st. The continuous depletion of soil moisture after the last date brought it down to about 5 per cent on the ridges by July 1st. Winter temperature might fall to $-1°$ C. while a maximum of $104°$ C. in September and in October has been recorded. This occurrence of two peaks of high temperature was due to the occurrence of summer fogs, which equalized temperatures and illumination; minima of $6°$ C. have been registered in July, which, it will be noted, is only 7 or $8°$ C. above the minimum of winter.

No further measurement of soil factors than that indicated above was made. The chief mineral constituents in the two places were derived from granitic rocks and sands. So many features of soils are directly dependent upon meteorological factors that uncorrelated information concerning separate characters of soils have but little meaning, at least so far as the results described below.

The above conditions in the coastal plantation made for an indeterminate growing season, with very vivid contrasts between places watered by springs and streams and the slopes of nearby ridges. Deciduous trees such as the willow, of course, showed very definite and withal somewhat brief seasons of activity. Evergreen trees, especially conifers, such as the Monterey Pine might continue activity throughout the

year. The description of the growth of Monterey Pine No. 17 is believed to be the first authentic record of such action, although tropical trees have long been reputed to grow continuously.

The structure, form and general aspect of leaves, fruits and other parts of a leafy shoot are affected by variations in a number of factors contributory to its development. If for example a plant be placed under conditions which induce or permit any one of the several embryonic tracts which may be involved to display relatively higher rates, or which come into activity earlier or later than in the normal or accustomed sequence organs or tissues may be formed which present unusual aspects.

Furthermore hormones such as are supposed to emanate from awakening buds may be released prematurely and induce unwonted action in cambial layers and vegetative points. The possible disturbances which may result would make a long list. One of the most direct and obvious is the awakening of two processes simultaneously which ordinarily come one after the other. This is illustrated by the concurrent formation of wood and development of nuts in Juglans in a coastal habitat, while the leaf-products are chiefly converted to wood in the earlier part of the season in the habitat of the tree, and are later available for the rapid development of the nuts which do not appear until about the end of the period of wood-formation.

Nuts of *Juglans major* from trees at 8,000 feet in the Santa Catalina mountains in Arizona were germinated in the coastal location at Carmel, California in 1909, and made such rapid growth that they began to bear nuts in 1919. Measurements of growth and general observations on the seasonal procedure of these trees and of others in the original habitat were now begun.

A dendrograph was attached to the trunk of a tree in the montane location in May 1922. This tree was about 20 years old and had a trunk of 20 cm. in diameter. The leaves were unfolding and had exposed about half of their final area when

enlargement of the trunk began on June 6th. Flowers matured in mid-July after more than half of the growing season had elapsed and nuts were seen the first week in August after increase of the trunk had ceased. Its period of enlargement was 55 days and the total increase 2.5 mm. The program was therefore in sequence as follows: awakening buds, unfolding leaves, elongation of branches overlapping wood-formation in the trunk which was followed by development of flowers the nuts appearing after enlargement had ceased.

It is of great interest to follow this program through several years at the coastal location. The dendrograph attached to the tree in 1920 when it was 12 years old showed an initiation of enlargement of the trunk, May 13, which was 50 days after the leaves had begun to unfold, and was after they had reached full expansion. Extension of the branches had about reached its limit. Growth of the trunk began May 17, shortly before the flowers were mature, and nuts were seen June 1st only 18 days after the beginning of growth in the trunk. The period of enlargement of the trunk of 89 days resulted in an increase of 6 mm. in diameter.

The program in 1921 showed initiation of enlargement of trunk on April 18, maturity of flowers by mid-May, the appearance of nuts on June 10th, and the continuance of growth of the trunk for 165 days with a total increase of 10 mm. in diameter.

The initiation of growth in the trunk was much later in 1922, not showing until May 18th well after leaf maturation and extension of the branches which had begun the first week in April or about 50 days earlier. Increase of the trunk continued for 120 days with a total accession of 12 mm. in diameter: 16 nuts were produced.

No dendrographic record was made in 1923 but cores taken from the trunk showed that layers about equivalent to those of the two previous years were laid down. Buds awakened at the end of March and 20 nuts were produced.

The season of 1924 was characterized by a maximum of sunshine and a minimum of fog, which conditions were more

nearly like those of the original habitat and 110 nuts were collected.

In 1925 the dendrographic record was not begun until June 29th when the season was more than half gone. Enlargement ceased on July 30th with an observed increase of 1 mm. in diameter during the month. A growing season of 80 days to 90 days for the trunk is indicated. This brevity is to be attributed to the deficient water supply. 60 nuts were collected.

The leaves began unfolding March 19, 1926. Their expansion and the elongation of the branches was about complete before enlargement of the trunk began on April 11. Activity continued until August 1, over a period of 112 days with a total increase of 4 mm. in diameter. Enlargement of the trunk began on May 9, 1927, and continued for 136 days terminating September 20 with a total increase of 3.5 mm. 92 nuts were collected.

The leaves began to unfold about the first of April in 1929 reaching their full spread two weeks after enlargement had begun in the trunk, which occurred on May 13th. Enlargement ceased on August 1st after a growing period of 83 days the shortest yet displayed in this region. Five nuts were collected. Some of the leaves were retained until the end of November. A few were in a greenish condition and seemed capable of photosynthesis. The activity of the leaves extended over a period of 8 months. The total increase in diameter of the trunk was 2.6 mm.

The diameter of the trunk was now 220 mm. at the place of dendrographic attachment. Of this about 25 mm. was made of the greenish bark. It is to be said that while the bark begins to flake in trees at a diameter of less than 100 mm. in the native habitat, that here it remained active for a much longer period. A net increase of 10 mm. of the total thickness may be ascribed to the action of the bark.

It is to be noted with regard to the climatic factors that the season of growth and other activities of the walnut in its native habitat is coincident with that of the summer rains,

while the amount of precipitation at the coastal location during the growing season is negligible. The water supply in the last named location is from soil with a diminishing supply except as maintained by seepage from springs. The thickness of the layers of wood formed together with the rainfall arranged for the calendar year and also for the year ending in the mid-summer dry season is given below:

Thickness of Wood Formed			Rainfall for Calendar Year	Rainfall for Year Ending July 1
	Juglans	Rinus		
1920	6 mm.	—	—	—
1921	10	—	—	—
1922	12	—	573	495
1923	—	30.5	349	243
1924	—	48.0	208	270
1925	—	46.1	410	375
1926	4	46.2	414	516
1927	3.5	30.2	627	630
1928	—	32 1	481	626
1929		37.0	303	473

The important divergences from the program of the tree in its original habit included a long period of leaf-activity and extension of branches before wood formation in the trunk began. While increase in the trunk was seen in a few days after the leaves began to unfold in the montane area as much as 50 days might elapse after bursting buds in the coastal area before increase in the trunk was seen. An advance in flower and fruit development was noted. Flowers developed late in the growing season on the mountain and nuts were visible only after growth of the trunk had ceased. Flowers matured in the first part of the growing season of the trunk at the coastal location and nuts appeared within three weeks after enlargement of the trunk had begun.

The products of the first 50 days activity of the developing leaves seemed to be used in self-construction and in the extensions of the branches on which they were borne. The staminate flowers arise from the axils of the leaves of the previous year. It was not until the new branches were fully extended and the

leaves fully expanded that development of the inflorescences began running coincidentally with the laying down of wood on the trunk.

The period of leaf activity is greatly increased in the coastal location, and might be as much as four times as in the original habitat. The period of enlargement of the trunk was never less than 50 per cent longer than in the montane area and sometimes was three times as long.

The general aspect of the leaves was markedly altered by the prevalence of the maximum number of leaflets on a greater number of leaves. A statistical study of hundreds of these organs would be necessary to secure results of definite value. The characteristics of the nuts developed in the two places are more easily to be defined.

It has already been noted that the flowers mature much earlier in the season at the coastal location and that nuts begin to show early in the growth of the trunk. Some features of the growth of the nut have been previously described (Publ. 350, Carnegie Inst. of Wash., 1924, p. 50).

Upon consultation of the original auxographic record, from which an account of the growth of a nut was published,[1] it was found that a nut was 16 mm. in diameter on July 3d at which time the auxographic record was begun, and that enlargement continued until Aug. 24 so that the observed period of enlargement was 52 days. It was estimated that the nut was 10 days old at the beginning of the observations so that the period of development of the nut was at least 62 and may have been as long as 70 days. The nut increased its volume eight times during the period in question. The development of the nut in the original habitat of the plant is mainly within the month of August and does not extend over half the number of days during which enlargement goes on in the coastal location. As has been noted, however, the enlargement of the nut in the montane habitat is from material for which there is no competition by wood-forming processes.

The development of the nut in the original habitat did not

[1] Publ. 350, Carnegie Inst. of Wash., 1924 to 50.

begin until after wood-formation had ceased. The entire development of the nut auxographed at Carmel took place within the period of growth of the trunk which according to the dendrographic record began May 18 and continued until September 15. It is to be seen therefore that a competition between the cambium derivatives and the nuts for the leaf-products was carried on during the entire period of development of the nuts. In the original habitat wood is formed during the first half of the season and the nuts do not begin to enlarge until this is finished.

In addition to the disturbance of correlations between the activities of various pairs and groups of organs and in addition to the direct competition for the carbohydrates formed during any period the importance of the carbohydrate-nitrogen ratio is well known. About double the amount of wood is formed during a season at the coastal location that is added to the trunk of a similar tree on the mountain.

According to the results of Kraus and Kraybill, relatively high nitrogen content of nutritive solution retards reproductive action, while relatively high carbohydrate formation increases flowering and fruit formation. As carbohydrate formation depends directly on light exposures, length of daylight periods and other variants it is not possible to make any exhaustive analysis of the changes which occur when the walnut is transplanted to the coastal location. It is to be noted however that the early formation of leaves and their continued action through a longer season would tend to an accumulation of sugars and to a heightened ration of carbohydrates to nitrogen early in the season. Flower formation being consequently induced early in the season and continuing concurrently with the formation of woody tissues. No statistics of any great value are available but it does not appear that the nuts formed are any larger in the coastal location with the longer season or that any greater number are matured; neither can anything definite be said as to the relative number of flowers which are aborted in the two cases.[1]

[1] See Maximov, N. A., "Experimental Aenderungen der Länge der Vegetationsperiode bei den Pflanzen, Biol. Zentralb., vol 40, Hft. 9, 513–543, 1929. Also Kraus

The net total result of the lengthened season of activity of the photosynthetic organs is an increased production of wood but not of larger nuts or a greater number of them so far as general observations be relied upon.

Among the more obvious differences between the nuts formed in the long season in the coastal location and those of the short season in the montane habitat was that those formed in the last-named location were deeply furrowed, while those of the coastal location were only slightly rugose. This feature was invariable and makes for an appearance of plumpness of the nuts formed in the longer season although they are on the average not larger. Connected with the above is the fact that the nuts of the coastal location are longer by the addition of some material at the apex, so that they are prolate while the base presents a full outline when the nut is divided into the two halves into which it splits most readily.

From the X-ray images of the half-nuts it is evident that nearly twice as much material is laid down in the apical region of the coastal nuts as in those formed on the mountain. The half-nuts from the mountain show a base distinctly cordate while those of the coast are full, but this distinction is masked in the X-ray images. Finally the montane nuts are compressed-globose while those from the coastal region are elongated or ovoid.

Pine tree No. 17 of the dendrographic series of the Monterey pine has furnished one of the most interesting records yet made of the growth of any organism. This tree stands on the margin of an old quiescent dune not far from the shore of the Pacific in the margin of a swampy strip fringing a small stream fed by springs. There is consequently an adequate water supply at all times while the relative humidity is generally high except for a few hours during the year.

A dendrograph for recording the changes in the diameter of the trunks a meter above the base was installed October,

and Kraybill, "Vegetation and Reproduction with Special Reference to the Tomato, Bull. Oregon Agric. Exper. Sta., No. 149, 1918, pp. 1–90. Nightingale, D. T., "The Chemical Composition of Plants in Relation to Photoperiodic Changes," Res. Bull. Wisc. Agric. Exper. Sta., No. 74, 1927, pp. 1–68.

1922. The tree at this time was 8 meters in height, 16 cm. in diameter and was estimated to be 16 years old. The bark had begun to flake on the basal part of the trunk but was still smooth, green and turgid at the points of contact of the instrument when the measurements were begun.

It would seem that the tree was at the stage at which the rate of increase in thickness of the trunk was rising toward the maximum very steeply. The accretions during the following years are shown on p. 10.

The total increase in diameter at the base during these seven years was 270 mm. the rate apparently falling irregularly from the maximum which was reached at the age of 18 to 20 years (1924–1926). The more important matter of actual volume of wood formed will be taken up in a paragraph below.

It is remarkable that this tree reached the maximum growth rate in a season of minimum rainfall following a season in which the precipitation was below the average. As noted above it stood in the moist margin of a dune and the roots reached an adequate supply of water at all times. The scanty rainfall was accompanied by diminished fogs, greater number of hours of sunlight, higher temperatures of the air and presumably greater number of hours of optimum temperatures of the cambium. These conditions would of course result in the formation of a greater amount of wood.

It has been asserted (Rep. of Conference on Cycles, Carnegie Inst. of Washington, 1929, pp. 30, 31) that during the first six years a positive stoppage of growth as manifested in enlargement of the trunk occurred but once, and that at the end of December 1926.

This statement was based on a summation of the dendrographic record taken in fortnightly periods. If an increase had taken place within this time no stoppage was denoted. A pause occupying the greater part of the bi-weekly period might thus pass unnoticed. A closer analysis of the behavior of the tree from the beginning of the record October 23, 1922, to March 1930 was undertaken in connection with the present studies.

Much has been said in previous publications concerning reversible variations in diameter of stems especially trunks of trees. The tensions set up in the continuous meshwork of water in the vessels and woody elements may exert such a pull that an actual contraction of the non-living elements may occur. By recent observations made with dendrographs bearing on the inner parts of the woody cylinder of the tree the shrinkage may amount to as much as one part in a thousand. This occurs when the loss in excess of the rate at which it may be conducted to the leaves by the woody cells. The contraction must be greater in the woody layer of the previous year and greatest in the layer to which additions are being made from the activity of the cambium.

Any diminution of the rate of water loss as at night or by reason of high relative humidity, fogs, rain, etc. lessens the tension on the hydrostatic system and allows the elements to return to their normal volume. Ordinarily a contraction occurs on days of uninterrupted sunshine followed by expansion at night, these being the two phases of the "Daily Reversible Variations." Extreme and continued loss of water may be followed by shrinkage continuing day and night for some time while rains may be accompanied by increases uninterrupted for several days.

The effect of a water deficit and the accompanying increased tension of the hydrostatic system on the hydration swelling of the cambium initials, derivatives and in the maturing woody cells which still retain their plasmatic contents is somewhat complicated. The reduction of hydration would of course be followed by some decrease in volume of elements which were in their final form. The increased tension of the hydrostatic system would be of a magnitude which might result in lessened turgidity in all vacuolated cells, coincidental with an increase of suction power by reason the heightened concentration. These conditions would tend to retard the distention or increase in volume of both daughter cells and of their final derivatives. What effect the increased tension of the hydrostatic system would have on the division

of the dense masses of plasmatic material in the cambial initials can not be described.[1]

The other source of interruption to the growth of the Monterey pine in its native habitat would be low temperatures. When the cambium falls to 7 or 8° C. the dendrographic record shows a cessation of enlargement but no shrinkage unless caused by winds which might set up great tensions in the hydrostatic system. Here again no analysis can be made as it is not known whether both division and distention are affected.

The first period of disturbance of the progress of growth of pine tree No. 17 which has made a record of growth as nearly continuous as any which may be shown by a tree or plant of any kind began on Jan. 25, 1923, when the net shrinkage of the trunk was 0.5 mm. then equal reversible variations occurred for two days, followed by a further slight decrease for four days at the end of which the tree was 0.8 mm. less in diameter than at the beginning of the disturbance. Four days of equilibrium were followed by an increase which continued for many weeks—until another disturbance occurred beginning December 19, 1923.

The interruption at this time took the form of a very slight shrinkage which in three days diminished the diameter of the trunk 0.3 mm. Equilibrium of expansion and contraction followed for three days then some increase took place. On January 3, 1924, a wavering record began which registered some increase by January 14, 1924.

After the above date no pause in the continued increase of the trunk was seen until nearly three years later. On November 30, 1926, a slight shrinkage began which lessened the diameter 0.2 mm. in two days. This was followed by a period of 36 days in which the variations were reversible daily with occasional periods of shrinkage for two or three days or gains for the same length of time. The temperature of the air fell

[1] See Ursprung und Blum, "Eine Methode zur Messung des Wand-und Turgordruckes der Zlle, nebst Anwendungen," *Jahrb. f. wiss. Bot.*, vol. 63, Hft. 1, pp. 1–110, 1924, esp. p. 53, 54, and Walter, H., "Der Wasseraushalt der Pflanze in quantitativer Betrachtung," *Naturwissenschaft und Landwirtschaft*, Hft. 6, 1925. See p. 91.

as low as 30° F. and although no true temperatures were recorded it is certain that the cambium was retarded by cold.

On January 10, 1927, a slight increase began accompanying elongation of the tips of the branches—but action was uncertain and wavering until the 18th when a shrinkage began which resulted in a loss of 0.6 mm. in diameter by January 25. After a week of equilibrium increase was resumed.

Next a disturbance began on November 21, 1927, by which irregular reversible variations occurred in daily, or two- or three-day alterations which continued for one month at the end of which time an increase set in which continued until December 4, 1928. A pause with uncertain action began on the above date which on January 16, 1929, was translated into a shrinkage by which the diameter was lessened 1.5 mm. in the next fortnight.

A rapid increase began January 25, 1929, which nearly balanced this loss, the total being 1.1 mm.; this increase was lost in five days before a steadied growth increase showed in the records.

Another pause began February 18, 1929, which lasted for a week only.

Mercurial thermometers were now installed to take temperatures under the bark in the cambium and derivative cells. Growth ceased November 19, 1929, and no net increase could be detected until the middle of January 1930. Minima of 7.5° C., 7, 6 and 6° C. were registered in November and December. The reversible variations were mostly daily with a few two-day periods. Maxima of 10 to 16° C. in the afternoons did not incite growth although the total shrinkage was very small.

On December 11 with no temperatures below 12° C. enlargement for three days occurred, but a pause followed and progressive winter conditions brought minima of 1, 4 and 5° C. during the first half of January 1930, with mid-day maxima of 11–13° C. with no growth. After January 16 the minimum did not fall below 8° C. and an enlargement began which initiated the growing season of 1930.

The above observations tend to show that the minimum of 8° C. may be regarded as the lower limit of temperature of the cambium under which accretions to the trunk may take place. On days when the temperature of the cambium falls to this grade for a few hours only in the morning no increases are to be recorded. What goes on during the great part of the daylight period with the temperature up to 11–13° C. is problematical, but daily recurrence of the minimum noted inhibits enlargement.

After the above conclusions had been reached confirmatory observations were made when the cambium fell to 8° C. on March 15, 1930, and on March 22 when a pause ensued following a record of 8.5° C. which may not have been the actual minimum. Supporting observations were made on Pine tree No. 1, in which temperatures of 9–9.5° C. checked growth, which was stopped when the cambium fell to 8° C.

The principal conclusions established or supported by the observations described above are as follows:

1. The generative layers of cambium of trees show no rhythms coincident with seasonal activity.

2. A condition of inactivity of living matter results only from the failure or deterioration of organs such as leaves or roots, with a resultant deficit in water or nutritive material.

3. Continuous activity is possible and may be expected in trees of the evergreen habit which bear several annual suites of leaves.

4. The Monterey pine has been the subject of detailed observations for 12 years; individual trees in especially favorable locations may continue growth without cessation for more than one season.

5. Pine tree No. 17 of the dendrographic series has been seen to make continuous additions to the trunk for a period of three years and to make but slight pauses in four other seasons.

6. Tree No. 17 is in an environic complex in which the only factor which so far has interrupted growth of the trunk is that of temperature. Growth does not take place at temperatures of the cambium below 8° C.

7. Deciduous trees. Juglans native to a region with short summers and severe winters, transplanted to a place with equable climate favorable to activity, shows a period of survival of the leaves and of growth activity two or three times as long as in the original habitat.

8. The seasonal program in the original habitat is a linear series as follows:

Leaf-formation on branch extensions—woody accessions to trunk-development of flowers and fruit—Leaf-fall.

The program in the longer season is a telescoped or forms are overlapping series as indicated.

Leaf and branch formation—Wood-formation—Leaf-fall.

Development of flowers and fruit.

9. Development of the inflorescences and maturation of fruit do not begin until the woody layer on the trunk is nearly completed in the original habitat. This action is usually coupled with a high soluble ratio of carbohydrate to nitrate in the food supply.

Such a ratio would be possible with the cessation of the formation of wood-cells.

The development of inflorescences with leaf-formation in the equable climate long season location suggests an early attainment of a high ratio of C to N.

10. The dislocation of the morphogenic program in the long season is accompanied by anatomical variations. The nuts of Juglans change from a compressed globose to an ovoid form, from a highly rugose shell to one approximately smooth with an excessive amount of material in the apical part of the nut. These and other features will be discussed in a separate paper.

VERGIL AFTER TWO THOUSAND YEARS

By JOHN C. ROLFE

(*Read April 25, 1930*)

THE excuse for this paper is that it seemed to some of us appropriate that the American Philosophical Society should join with other learned bodies in this country and abroad in recognizing in some way the bi-millennial anniversary of the birth of the poet Vergil,[1] to whom many nations are this year paying honor.

That the sum of 70 and 1,930 is 2,000 will hardly be questioned, but since A.D. 1 followed 1 B.C. without the intervention of a year "zero," it seems that the two thousandth year after 70 B.C. is 1931 and not 1930, a fact which has not escaped the notice of all classical scholars.[2] The error of a whole year in two thousand will doubtless seem a particularly serious one to our scientific colleagues, in whose eyes a millimeter is as a day's journey and an atom is a universe. We can plead in our defence only that the date 1930 was set as long ago as 1924 by the Italian society Atene e Roma and has been generally adopted, so that a change at this late hour does not seem practicable.

This paper has been a hard one to write, not for lack of material, but rather because of the difficulty of sifting out from a mass of detail anything really significant that could be said in twenty minutes. There have come down to us under Vergil's name three great literary masterpieces: ten pastoral poems, commonly known as *Eclogues*, or "Selections," but more properly called *Bucolics;* the *Georgics*, a poetical treatise on farming in four books; and the *Æneid* in twelve books.

[1] The classical spelling of the poet's name was *Vergilius*, which in late Latin became *Virgilius*, perhaps under the influence of *virga*. Whether one writes Vergil or Virgil in English is purely optional; cf. Mithradates and Mithridates, Lepcis and Leptis, etc. The variations between the two spellings in this paper do not represent inconsistency on the part of the writer, but the varying usage of the authorities cited.

[2] See J. K. Fotheringham, *Class. Rev.*, xliv, pp. 1 ff.

Of the authenticity of these works there has never been any question, but as to a number of minor poems, supposed to be youthful works of Vergil,[1] a great deal has been written. Some scholars reject them all, others accept them in great part, or even wholly; still others admit the genuineness of a few and deny that of the rest. The last seems the safest course; however, the question has no bearing whatever on the poet's fame, is of slight interest for the history of Roman literature, but is of considerable moment in connection with Vergil's biography. Each poem has been examined from almost every possible angle, but there seems to be little probability that anything like general agreement will ever be reached.

In addition to the scanty information given in Vergil's genuine works, and that assumed to be given by some of the minor poems, we have several brief biographies from Roman times. Around these also a considerable literature has gathered, discussing the relative value of the *Lives* and the amount and the source of the interpolations and alterations to which they have been subjected. Here again general agreement has not been reached, but the *Life* by Suetonius, of the time of Hadrian, and the one attributed to Probus, of the reign of Nero, are commonly regarded as the best. Both these sources must be used with caution, but in not a few instances modern writers have given free rein to their imaginations. To cite but a single example, one eminent scholar makes a statement that will delight the advocates of painless and effortless education, namely, that Vergil hated grammar.[2] I know of nothing on which such a dictum can be based,[3] and

[1] The full list is as follows: *Catalepta, Priapea*, Epigrams, Ciris, *Culex, Moretum, Copa*, Lydia, Aetna, Dirae; those printed in italics are most commonly regarded as youthful works of Vergil, which he did not intend to publish.

[2] N. W. De Witt, in *Vergilian Papers* published by the Service Bureau for Classical Teachers, Bulletin XV; New York, 1929, p. 16.

[3] The source of the statement may perhaps be the translation by Conway in *New Studies of a Great Inheritance*, London, 1921, p. 100, of *Catal.*, vii, 3 ff.:

"And you, dull tribe of ample waist,
Whose barren joy it is to hammer
Young heads with ding-dong rules of grammar."

The Latin reads:

Et vos, Selique, Tarquitique Varroque,

if it were true, Vergil would deserve the greater praise for recognizing the value of the hated subject; for he gave it sufficient attention to gain unparalleled mastery of the resources of his native tongue, and to be cited in later times as an authority on grammatical usage.

Our poet was born in a district (*pagus*) called Andes, between Mantua and Cremona, but the exact location of the place offers a third problem. Its solution depends in part on the relative credibility of the Suetonian *Life*, which puts Andes "not far from Mantua," and of the *Life* by Probus, which says that it was thirty miles from that city; unless perhaps we may assume that Suetonius regarded thirty miles as no great distance.[1] It also depends somewhat on the interpretation and application of casual references to Vergil's farm in the *Bucolics*.[2] The Italians generally have accepted the view held both by Dante[3] and by Boccaccio,[4] that Andes was on the site now occupied by the modern village of Pietole, and they appear to resent any attempt to question this tradition.[5] It is nevertheless disputed by several scholars, both on account of the statement of the distance in the *Life* by Probus and especially because of the nature of the country at and around Pietole, which does not correspond with the allusions in the *Bucolics*. It has been located opposite Valeggio, fifteen miles north of Mantua on the west bank of the

> Scholasticorum natio madens pingui,
> Ite hinc, inane cymbalon iuventutis.

Further comment seems superfluous, but it may be added that according to the lexicons, including the *Thes. Ling. Lat.*, *cymbalon* refers to rhetorical studies, so that a far more accurate translation is given on p. 6 of the *Vergilian Papers* cited in note 2, above:

> "Ye silly tinkling cymbals whose dull breath
> Drivels the poor young fellows to their death."

Still better is the rendering of the *Loeb Classical Library* (where the poem is numbered V): "ye empty cymbals of our youth," which is really all that Vergil says. The mention of a Varro is hardly enough on which to base a shibboleth for those who suppose that the Latin masterpieces can be appreciated without a sound knowledge of the language.

[1] Cf. W. S. Gilbert, *Bab Ballads:* "Twenty-seven is not very old."

[2] E.g., ix, 7 ff.

[3] *Purgatorio*, xviii, 83.

[4] See *Class. Phil.*, xxv, p. 30.

[5] See Conway, *Vergilian Age*, p. 22.

Mincio, and at Calvisano, which is just thirty Roman miles (twenty-eight English) from that city on the river Chiese.[1] Both locations offer difficulties and the question is still *sub iudice.*

One more problem may be mentioned, although it also does not in the least affect Vergil's fame, namely his nationality. As a native of Cisalpine Gaul, he might well have been of Etruscan or of Celtic origin; or he might have been of pure Italic stock, the son of a colonist who had settled in that region. The first two opinions have had their champions, who have drawn from them, especially from the second, interesting but futile deductions as to the poet's temperament. But there seems to be good evidence that he was of Italic origin, sprung from that hardy and virtuous Sabine stock which had produced Varro and was to give birth to the emperor Vespasian.

It is clear that without going into the minutiæ of interpretation and text-criticism, there are problems enough connected with the life and works of Vergil. None of these, however, seemed quite suited to the occasion, and time does not allow a full account of the poet's productions with the reading of illustrative selections. I must content myself with a very brief survey of the unquestioned facts of his life, followed by an equally brief consideration of some of the reasons which have led to the celebration of his natal day after an interval of twenty centuries.

He was born, then, somewhere in the neighborhood of Mantua and Cremona on the fifteenth of October in the year of the first consulship of Pompey and Crassus.[2] His father was the hired man of a certain Magius, whose daughter he afterwards married. The *Life* by Probus says that Vergil was reared in humble circumstances, the Suetonian *Life*, that his father had greatly increased his little property by bee-keeping and by buying up woodlands.[3] At any rate, he was

[1] See Conway, *Vergilian Age*, especially p. 38, note.

[2] 70 B.C.

[3] This sentence is regarded as an interpolation by R. M. Geer, *Harvard Studies in Class. Phil.*, xxxvii (1926), pp. 99 ff. It is said that Vergil's farm furnished land for sixty veterans.

able not only to allow his gifted son the usual education of a well-to-do-citizen, at Cremona, at Mediolanum, and finally at Rome, but to permit him to supplement it by a course of philosophical studies at Naples under the Epicurean Siro.

In the course of the confiscation of farm-lands for the benefit of Octavian's veterans after Philippi the poet lost his estate, but through the influence of powerful friends received compensation in the form of land in Campania and perhaps also in money. In the course of his appeal for protection he established friendly relations with Maecenas and Octavian, and through the patronage of the former was able to devote the rest of his life to literary pursuits, living for the most part in Sicily and Campania, which were more favorable to his health than the city of Rome.

After writing the *Bucolics* and the *Georgics* and finishing a first draft of the *Æneid*, in his fifty-second year he determined to travel in Greece and Asia Minor and spend three years in revising his epic, after which he planned to devote the rest of his life to the study of philosophy. These plans he did not carry out; he met Augustus in Athens and was persuaded to return with him to Rome. But he fell ill of a fever during a visit to Megara on a very hot day, aggravated the disease by continuing his journey, and died at Brundisium on the twenty-first of September in the year 19 B.C.

The *Bucolics* struck a new note in Roman poetry and established Vergil's reputation; the *Georgics*, on which he spent seven years of loving labor, contain in their descriptions and digressions some of the poet's very best work; but it is in the *Æneid* that his genius is most clearly evident, and it is through the *Æneid* that he is known to many generations of our countrymen, in common with the educated men of most European nations.

The Romans had a decided taste for epic poetry, especially when it dealt with the history of their country. The first work of their literature with which we are acquainted was a translation of the *Odyssey* into the native Saturnian metre by Livius Andronicus, a Greek freedman from Tarentum. Even

before Livius the germ of the historical epic, as well as of the biographical literature which during the two centuries between Tacitus and Ammianus largely supplanted history, is to be found in the songs in honor of distinguished men and their houses, of which Macauley gave the atmosphere and the spirit, though of course not the form, in his *Lays of Ancient Rome*.[1] The *Odyssey* of Livius was soon followed by the *Bellum Punicum* of Gnæus Nævius, also in Saturnians, giving an account of the first Punic war, in which the author served as a soldier, but beginning with the fall of Troy and the coming of Æneas to Italy. Not long afterwards the *Annals* of Quintus Ennius became the national epic of the Romans, and held that position until the publication of the *Æneid*. Ennius also began with the coming of Æneas and continued the narrative into his own times, but he wrote in the heroic measure of the Greeks, the dactylic hexameter. We have mention, but no remains, of other pre-Vergilian epics dealing with historical themes. Many Roman poets had the desire to celebrate their country's history, and many public men were eager to be immortalized in their writings.[2] Vergil is said to have thought of such a work in his earlier years, perhaps inspired by the deeds of Julius Cæsar; but he wisely postponed it until he should be thoroughly prepared to undertake the task. It was an undertaking from which one might well shrink and for which Horace had expressed his unfitness.[3] An epic not only demands a mighty theme, which more than one event in the history of Rome might supply, but it must show the working out of a providential purpose and, by epic convention, the direct participation of the divine powers. To represent the gods of the Græco-Roman pantheon as taking a hand in the events of the later days of the Roman republic would be as difficult as it would be to introduce the supernatural into a poem on the late war. In spite of the titanic nature of that conflict, abounding as it did in deeds of heroism and of tragic

[1] See "Suetonius and his Biographies," *Proc. Amer. Philos. Soc.*, lii (1913), pp. 217 ff.

[2] For example, Marcus Fulvius Nobilior took Ennius with him to Ætolia, to celebrate his campaigns; Cicero, *Tusc. Disp.*, i, 2, 3.

[3] *Satires*, ii, 1, 13 ff.

pathos, it would be a bold poet who would venture to make it the subject of an epic. One luckless man did write an epic on our war with Spain, and it was a lamentable performance. The Roman historical epics of Lucan [1] and of Silius Italicus [2] cannot be regarded as wholly successful, although there was more fitness in introducing the marvellous and the supernatural into the remote times of Hannibal than into the materialistic and sceptical days of Julius Cæsar. To have written a poem which took its place with the three or four great epics of all time was enough to secure immortality for Vergil.

As we have seen, the custom of beginning a Roman epic with the fall of Troy had become traditional, and Vergil wisely followed the example of his predecessors. He thus secured, as the background across which his characters should move, the twilight of legend and myth.[3] His long period of preparation had given him mature judgment and psychological insight, and to his own nature was due that deep human sympathy which so often strikes what Mackail happily called the "note of brooding pity."[4] It is this last trait which makes him feel for the misfortunes of men, even those of whom he disapproves, as well as of animals; it is this which enables him to present such striking pictures of filial and paternal affection and of devoted friendship. It is a new note in Roman poetry, which made Vergil's work epoch-making. This quality, as well as a philosophic recognition of the mystery of human destiny, are beautifully expressed in Tennyson's ode *To Virgil*, written at the request of the Mantuans in commemoration of the nineteenth centenary of Vergil's death:

"Thou that seest Universal
  Nature moved by Universal Mind;
Thou majestic in thy sadness
  at the doubtful doom of human kind."

[1] The *Pharsalia*, or *De Bello Civili*, a poem on the civil war between Cæsar and Pompey.

[2] The *Punica*, having for its subject the second Punic, or Hannibalic, war.

[3] Gummere, *Handbook of Poetics*, Boston, 1885, p. 10.

[4] *Latin Lit.*, p. 94.

History in verse, even with a supernatural background, is not an epic, and one of Vergil's most difficult problems was to weave into an heroic poem the great deeds of the Roman people and in particular those of Augustus. His design was patriotic, but his birth and training had saved him from the narrow and somewhat selfish point of view of the urban Roman. His was rather the broad outlook of Julius Cæsar, who was very free in enrolling new citizens [1] and was even suspected of intending to transfer the capital of the Roman world to Ilium or to Alexandria.[2] Antony too had conceived the plan of a great oriental empire,[3] but his relations with Cleopatra had made his designs even more repugnant to his fellow-citizens than those of Cæsar.[4] Augustus was more conservative, or more tactful, than his adoptive father,[5] although his attitude was more liberal than that of the average Roman noble.

Conformably to epic usage, the action of the *Æneid* is confined to a comparatively brief period, namely the last year of the Trojan hero's journey. We first meet him on his way from Carthage to Italy, apparently hastening to the speedy fulfilment of his hopes. The final efforts of Juno to thwart him make a tragic drama of absorbing interest. It is also in accordance with epic usage that the poem ends with the death of Turnus, leaving the reader to picture the sequel for himself, aided by such hints as the prophecy of Jupiter in the First Book,[6] the Curse of Dido in the Fourth,[7] and the like.[8] It is in this indirect way, without interrupting the flow of a tale of the Heroic Age, that the history of Italy and of Rome are introduced. Æneas receives constant assurance from dreams and prophecies of his arrival in the promised land. In particular, his visit to his father in the Lower World gives

[1] Suetonius, *Jul.*, xxiv, 2; xlii, 1; etc.
[2] *Ibid.*, lxxix, 3.
[3] Cassius Dio, l, 4–5.
[4] Cf. Horace, *Odes*, iii, 3, 37 ff.
[5] Suetonius, *Aug.*, xl, 3.
[6] i, 256 ff.
[7] iv, 612 ff.
[8] A recent writer in the *Classical Weekly*, xxiii, p. 173, criticises the ending of the *Æneid* unfavorably: it is largely a matter of taste.

him a belief in his divine mission and the favor of the gods which never leaves him. There, in a passage which has been called "Rome's Hall of Fame." Anchises points out the men who are destined to make Rome great, not mentioning them in monotonous chronological sequence, but as his eye might have fallen on them, concluding with the touching tribute to the young Marcellus.[1]

It is unfortunate that the last six books of the *Æneid* are less commonly read, for it is there that Æneas's mission is most evident, and it is there that the hero appears to best advantage. The greatness of Rome and the early history of Italy are skilfully introduced;[2] for example, when Vulcan at the entreaty of Venus has made Æneas a suit of divine armor, the shield is ornamented with scenes from Roman history, ending with the battle of Actium.[3] The poet concludes: "Such scenes as these on the shield of Vulcan, his mother's gift, the hero admires, and charmed with the portraiture of events of which he knows nothing, he lifts upon his shoulder the glorious destiny of his posterity."

Other devices by which Vergil redeems his poem from being a history in verse are by the use of elevated and dignified diction; he chooses less usual and more high-sounding[4] terms in place of the prosaic and commonplace, and uses archaic words and expressions sparingly and with good taste. In his efforts to avoid prosaic diction and give distinction to the language and actions of everyday life he is usually successful, and although sometimes the result seems strained and unnatural, he does not, as a recent writer has well said, "wrench and distort language, but exploits to the most for the sake of compression the nature of Latin thought-form."[5] It is this, combined with his habit of suggesting alternatives, and some other peculiarities,[6] that makes individual sentences and

[1] vi, 867 ff.
[2] vii, 647 ff.; etc; etc.
[3] viii, 627 ff.
[4] Cf. Horace's definition of a poet in *Sat.*, i, 4, 43 ff.:
Ingenium cui sit, cui mens divinior atque os
Magna sonaturum, des nominis huius honorem.
[5] W. F. J. Knight, *Class. Rev.*, xliv, p. 5.
[6] See Conway, *Vergilian Age*, pp. 51 ff.

phrases of the *Æneid* puzzles for commentators, although Vergil is usually clear and straightforward, and the resulting differences of opinion do not check the flow of the story. The *Æneid* is well adapted for use in schools, but fully to appreciate its greatness requires careful and thoughtful reading of the entire poem in years of maturity. It is needless to say that one's appreciation is greatly increased when it is read in the original, "the stateliest measure ever moulded by the lips of man." [1]

It is easy to select special passages of conspicuous merit, such as the dramatic account of the taking of Troy,[2] beginning with the sailing of Agamemnon's fleet from Tenedos, "tacitæ per amica silentia lunæ," and the flashing of the light as a signal to the treacherous Sinon. There are beautiful descriptions, such as that of the early morning departure from Epirus,[3] and dramas of heroism and pathos, like the episode of Nisus and Euryalus.[4] But, as Mackail says,[5] "it is in its total effect, and not in the great passages, or even the great books, that it seems the most consummate achievement."

Vergil readily lends himself to parody and to criticism, and both fell to his lot in Roman times; [6] to-day he perhaps suffers most from misguided attempts to "jazz up" his life and his works, thus depriving the schoolboy of one of the greatest benefits to be derived from the study of Vergil.[7] The character of Æneas and particular the story of Dido are often misunderstood. To judge a Roman poem one must gain as nearly as possible the point of view of a Roman, or at least must realize the force of Nepos' warning,[8] that different na-

[1] Tennyson, *To Virgil.*
[2] ii, 250 ff.
[3] iii, 512 ff.
[4] ix, 224 ff.
[5] *Lat. Lit.*, p. 101.
[6] See Suetonius, *Vita Verg.*, 43 ff.
[7] For example in *The Wooden Horse*, selection in *Vergilian Papers* (see n. 2, p. 258 above), p. 7: "We can picture the two poets on a now famous journey . . . to Brundisium. There are Horace with his black eyewash (a lotion for his weak eyes), and Virgil with his black headache, both of them snoring while Maecenas and his company hopped about after the tennis balls." This is supposed to give the sense of Horace, *Satires*, i, 5, 48 f.!
[8] *De Viris Illustribus*, Praef.

tions have different standards and practices. In brief, Æneas is himself a tragic figure, doomed to sacrifice his life to a great enterprise, to seek new lands when he longs for repose in a settled home, to wage wars when he desires peace, and to die before seeing the complete realization of his hopes. His desertion of Dido was from the Roman point of view a praiseworthy act of self-sacrifice, which would suggest a contrast with Antony's yielding to the charms of Cleopatra. The epithet *pius*—incorrectly, or at least incompletely rendered by "pious"—is thoroughly justified. When he cries out "sum pius Æneas," [1] it is not, as a recent writer points out,[2] "a piece of smug complacency," but an outcry against the gods for leading him on with promises and prophecies and then apparently abandoning him in spite of his devotion to them and their commands. So too in another passage [3] the same epithet reminds us of his divine mission and his obligations to his son and to his father.

I have been able in this brief time merely to touch upon a few points of interest, and probably my selection has not been the best possible. Doubtless some would have preferred to hear of Vergil's influence upon later times, including our own; [4] others, of the strange tales that gathered about his name in the Middle Ages and gave him a reputation for vast knowledge and acquaintance with astrology and the black art.[5] I have accomplished my purpose if I have to some extent revived the memory of a poet whom we have nearly all known and loved, and inscribed upon our somewhat prosaic annals our recognition of his bi-millennial anniversary.

[1] *Aen.*, i. 378.

[2] See *Class. Rev.*, xliv, p. 4.

[3] iv, 393.

[4] See Mackail, *Virgil and his Influence*, in the Series of "Our Debt to Greece and Rome."

[5] See Comparetti, *Virgilio nel medio evo*, 2d ed., Florence, 1896 (there is a translation of the first ed. by E. F. M. Benecke, London, 1895).

AN ELECTRO-CHEMICAL INTERPRETATION OF MEMORY

By GEORGE W. CRILE

(*Read April 25, 1930*)

SINCE the researches were initiated which were summarized in the Ether Day address on October 15, 1910, we have been endeavoring to apply physical laws in the consideration of biologic processes. The rapid advance in the physical sciences has made available many pertinent facts which could be utilized in medical and biologic research. Especially were we interested to find the trend to apply tangible and exact methods to the study of mental processes in the monograph on the "Physiology of Mind" by our President, Dr. Dercum, as attested by one of his concluding statements:

"The study of the recondite problems of human existence is in a sense a study that is imperative and should be pushed to its ultimate conclusions. Our knowledge of the constitution of the universe as revealed by the marvelous truths of radio-activity, of the structure of the atom and by the field opened up by Einstein's discoveries and theories is but an expression of this tendency; surely it should not be denied us in the study of the mind."

Any theory of memory based on physical laws must provide for a mechanism which will account for the persistence of memory throughout life. It must provide a separate mechanism for each individual memory; it must provide a mechanism for transmitting the memory patterns of species; it must interpret subconscious memory and dreams; it must provide for the temporary loss of memory in the individual under the influence of an anesthetic or a narcotic; it must show why memory varies with different bodily conditions such as senility, acute disease, hemorrhage, shock, general weakness, pain and emotional strain, loss of sleep; it must interpret such distortions of memory as are present in shell shock; it must account for the relative control of memory by the thyroid

gland—the increased acuity of memory in the early stages of hyperthyroidism in contrast with the low mentality of individuals whose thyroid activity is deficient. Finally the theoretical mechanism of memory must have the power of stepping up or amplifying the weak energy supplied by the sense receptors as exemplified by the reception by the eye of the slight variations in the light reflected from an object which produce an optical image which in turn results in a dramatic expenditure of energy.

To meet these manifold requirements, we propose that in the nervous system there are infinite numbers of electro-chemical mechanisms each consisting of one or more receptors, conductors, insulators, and energy-transforming units or cells. These mechanisms are specifically adapted to form and to conduct electricity, and to be stimulated by electricity, and are incapable of being operated by any other form of energy. These mechanisms we believe are competent to account for the phenomena of memory.

Such an electro-chemical theory is in harmony with various fundamental biologic facts, of first importance among which are—the constitution of the brain; the fact that the electric stimulation of nerve fibers and nerve centers makes the organs supplied by the nerves perform work identical with that which is produced by the direct stimulation of those fibers by the brain; the fact that oxidation is the principal source of the energy of the brain; the innumerable afferent fibers leading from each sense organ, and the innumerable microscopically visible fibers in the white matter, which of themselves alone, would make possible vast numbers of combinations.

Since this theory proposes that memory is electric in nature, then there must be adequate electric circuits in which it may operate. The bipolar or electric conception as to the organization and energizing of animals furnishes innumerable electric circuits. Every organ, every cell, every energy unit in the protoplasm according to this conception is in a circuit. Our theory proposes that when the receptor organs of the

senses are stimulated, electric or radiant energy is released and passes along the nerves to the receiving cells of the brain. Thus, for example, it has been shown by many observers that the light waves falling on the rods and cones of the eye cause electric energy to travel over the optic nerve. What mechanism is there in the rods and cones which causes them to produce electricity on the impact of light waves? The resultant electricity may be due to oxidation, or some stored electricity may be released as the result of a photoelectric effect, for in the rods and cones there is an abundance of potassium and of other metals in an ideal position for the ejection of a stream of electrons which would travel down the optic nerve as electrons travel from a photoelectric cell.

We can readily enough follow the electric impulses down the optic nerves to the brain, but where in the brain, and by what means does that feeble electric disturbance make a record so perfect and so durable that throughout life one may carry the memories of childhood, the nature of this mechanism being such that the image may be subjected to variations in clearness or may be temporarily obliterated by want of oxygen, by an anesthetic, by sleep, by hemorrhage, by pain, by emotion, by fever, by concussion of the brain, by an opiate, by senility, by loss of thyroid function?

We know of what material the mechanism that holds the image must be made, for the brain consists of water, various compounds of potassium, sodium, phosphorus, calcium and magnesium and a group of highly specialized fats. In other words, the brain consists of conductors and insulators.

All the billions of brain cells are in existence at birth for no new brain cells are added after birth. But few neurofibrillæ exist at birth and there is then but little memory except racial or species memory. As Cajal has shown, neurofibrillæ are developed and the development of the brain cells is completed and memory is established progressively during life. It would appear, therefore, that the nerve cells play a principal part in creating the organization of the memory mechanism and that the receptors of the sense organs stimulate the development of both the nerve cells and the neurofibrillæ.

The chemical constituents of the neurofibrillæ are essentially the same as those of the brain cells. Potassium and phospholipins appear to form a building pattern for the nerve cells and the neurofibrillæ, while sodium chlorid predominates in the fluids of the brain. Sodium would appear to be essentially a conductance electrolyte, while potassium would appear to be an organizing and structure forming element.

Let us now suggest a physical method by which an electric charge may convey and transfer in orderly fashion potassium-phosphorus-lipoid units from the brain cells to form fibrillæ and create a specific pattern.

The manner in which this may be accomplished is suggested by the manner in which an electric current may act upon potassium atoms in an electrostatic field.

Due to variations in the concentration of electrolytes within and without the cells of the brain, we must believe that electrostatic fields of varying intensity are everywhere present. Let us suppose that an electric impulse resulting from impacts on the sense organs strikes a membrane of a brain cell. The force of this current might be sufficient to separate from the potassium atom some of its electrons, the potassium ion becoming more or less positive according to the number of electrons it would lose. According to its degree of positivity this positive potassium ion would move for a longer or shorter distance in the electrostatic field, and as it moves, would pick up negative electrons lying free in its path, perhaps dislodged from other potassium atoms, until it had again acquired its full number, when it would remain quiescent until impinged upon by another electrical impact. Thus a succession of these potassium atoms would lose and take up again electrons, always moving further and further toward another cell, in which in turn, it would replace potassium atoms which had been similarly lost. Thus there would be a constant production of positively charged potassium ions with a constant replacement of neutral potassium atoms.

Let us suppose the film of a brain cell to be charged up positively in relation to a muscle or gland the fiber of which

has been divided. The charge reaching a certain potential might, by the process described above, produce a path of potassium atoms which would have the property of holding in its field other atoms, notably phosphorus, the phosphorus in turn possessing the power of holding complex lipoid molecules. A deficit of potassium atoms in the field of a cell would be replaced instantaneously from elements within the cell. Thus the nerve cell, without changing its structure, could, as it were, 'secrete' a nerve fiber or neurofibrilla, could repair the divided nerve, could create the white matter of the brain out of its own substance. Out of sufficient numbers of primary units of potassium-phospho-lipins, the essentials of the solid parts of a nerve fiber may be formed, leaving out of consideration the electrolytic element in the axis cylinder. Thus in a loose sense, we postulate that the nerve cells, as it were, 'secrete' the nerve fiber in the fetus, the infant, the growing child, and the man. When another like impact upon the same sense organ occurs the same neurofibrillæ would be stimulated and the pathway intensified, thus recreating the original image—creating memory. These infinite pathways thus created intercommunicate, allowing for infinite numbers of associations by any of which any original memory pattern may be stimulated and intensified, thus making possible the processes of thought and of reasoning, all of which are based fundamentally upon the original patterns established by the senses and the species patterns which are present at birth. As proof of this, it is necessary only to watch the manner in which an infant receives its first impressions of the world about it and slowly begins to have memory patterns, the memory patterns in turn giving rise slowly to the faculty of reasoning. Thus nerve cell activity results in memory, thought, and emotions, no less than in muscular action or glandular action.

That the brain is governed by physical laws has been established by researches carried out by Miss A. F. Rowland, Dr. Maria Telkes and myself. We found that the conductivity, potential, capacity and oxidation varied with the physical state of the individual. That is, that in conditions

in which memory was more or less acute there was a correspondingly increased or diminished oxidation, conductivity and capacity. Of special significance has been the finding of G. H. Crile of the fact that the white matter of the brain has a high capacity for holding electric charges.

In experiments on rats we have been able to produce facilitated pathways both in freshly removed brains and in living brains by the passage of an electric current, this facilitation being manifested by increased conductivity along the path of the stimulating current and diminished conductivity across it (Fig. 1).

Recently we have happily found evidence which tends to support our above-stated belief that the intercommunicating pathways in the brain are 'secreted' by the brain cells. Miss Rowland, Dr. Telkes, and I have found that by taking the separate constituents of a brain—the phospho-lipins and the salts contained in brain ash—we could produce the equivalent of neurofibrillæ by the application of an electric charge, thus, as it were, reorganizing the brain elements by electricity. Under the microscope we could see lines of force resembling neurofibrillæ connecting two small masses of these mixed brain elements (Fig. 2*b*). Under the microscope these fibrillæ could be seen to form rapidly—the growth extending toward the positive pole (Fig. 2*c*). All through this mass of brain substance lines of organization could be seen. Of special interest and significance was the observation that the same general causes of depression of memory in man, such as an anesthetic or a narcotic, lessened or interfered with the organization of the brain material (Fig. 3), while the addition of potassium iodid corresponding to thyroid secretion increased this organization (Fig. 4).

Moreover, this organized material possesses a fairly high capacity and it has the power of oxidation, as shown by measurements in a Warburg respirator.

Measurements of the potential of models made of this brain mixture combined with collodion showed a potential which varied with the electromotive force of the current ap-

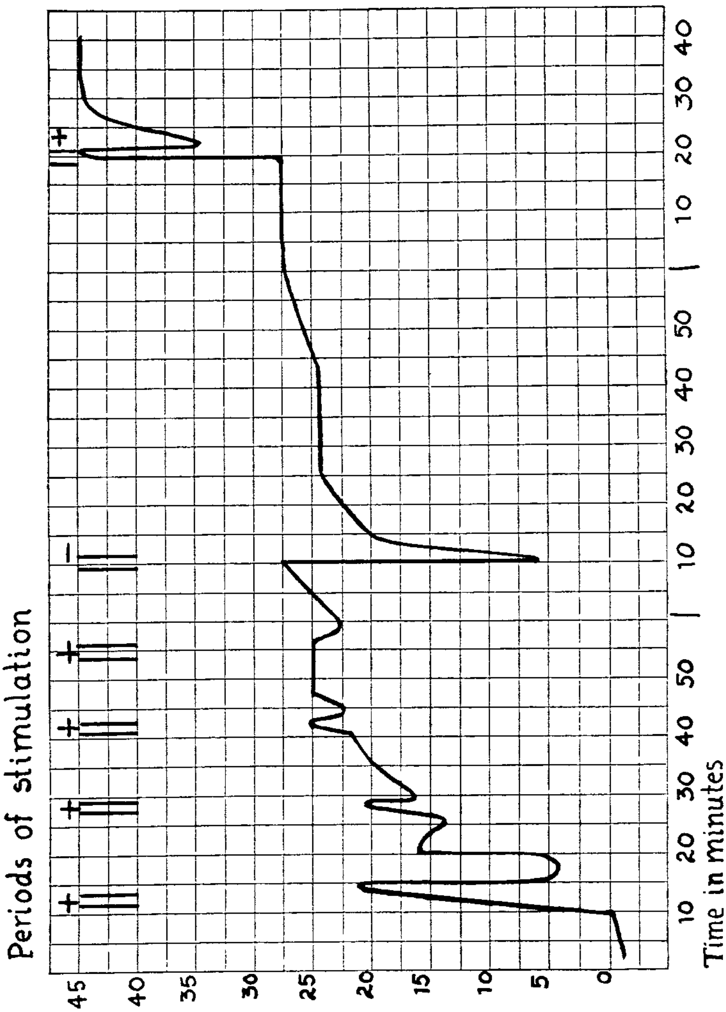


FIG. 1.—Percentile changes in the conductivity of the brain due to electrical stimulation. Note the apparent facilitation of the path between the electrodes on the passage of repeated positive charges, and the decreased conductivity when a negative charge was passed.

plied, and a lowered potential in the presence of narcotics or anesthetics. Of special interest and importance was the finding that the potential of these lipin-collodion mixtures varied with the concentration of the electrolytic solution in which they were immersed, in accordance with the Nernst formula for concentration cells, and that in the passage of electric currents through these membranes the polarizing or response current or 'sensation' is a logarithmic function of the polarizing current or 'stimulus,' in accordance with another Nernst formula. All of these findings correspond to the effect of the changes in electric potential which cause depression of memory in living animals.

In addition to the method of electric organization described above, the process of creating and of energizing memory patterns could be conceived as being due to photoelectric effects such as we have suggested are produced by the rods and cones. In the case of the eye, the frequency of the waves of light is obviously adequate. But we know that there may be high frequency waves everywhere, for Gurwitch has demonstrated such high frequencies in growing roots of plants and in animal tissues, and Siebert has demonstrated that these radiations are due to oxidation. Of special significance is the finding of Potozky that the cyanides which inhibit oxidation inhibit the Gurwitch radiations and we have found that in our artificial brains the cyanides inhibit the production of fibrils. Certainly cyanide inhibits memory and life in man.

Dr. Otto Glasser of our Physics Department suggests an analogy in the effects of the x-rays on tissues. From a heated metal electrode electrons are ejected forcibly against a metal target in the x-ray tube. At the point of impact these high speed electrons are stopped and their energy is transformed into electromagnetic waves. These waves travel to the living tissues and there eject photoelectrons and recoil electrons from the atoms in the living tissue. These electrons set up new radiations until finally the energy is spent. In this way great biologic changes are produced by relatively small amounts of the primary energy of the x-ray beam. In like manner, on

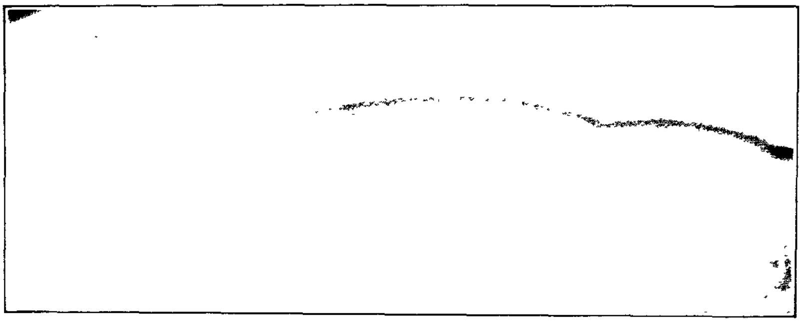
(*a*) Dry drop of phospho-lipin.

(*b*) Drop of phospho-lipin in brain-ash solution. Notice the two masses of phospho-lipin connected by fibers.

(*c*) Fiber formed upon the application of an electric stimulus to a drop of phospho-lipin in brain-ash solution. Note the tendency of the fibers to extend toward the positive pole.

FIG. 2.—Formation of artificial fibers from phospho-lipins of the brain. (From photomicrograph × 200.)

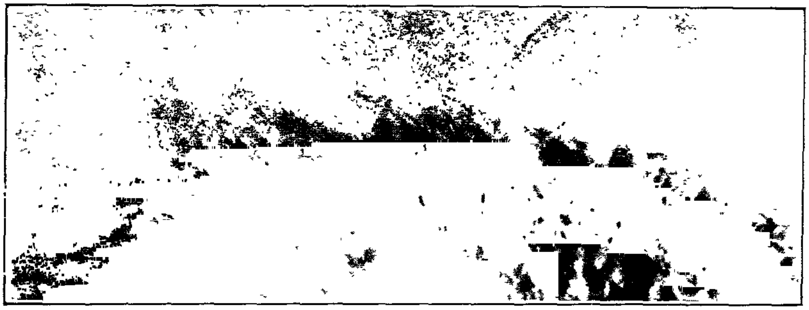
(*a*) Drop of phospho-lipin in brain-ash solution.



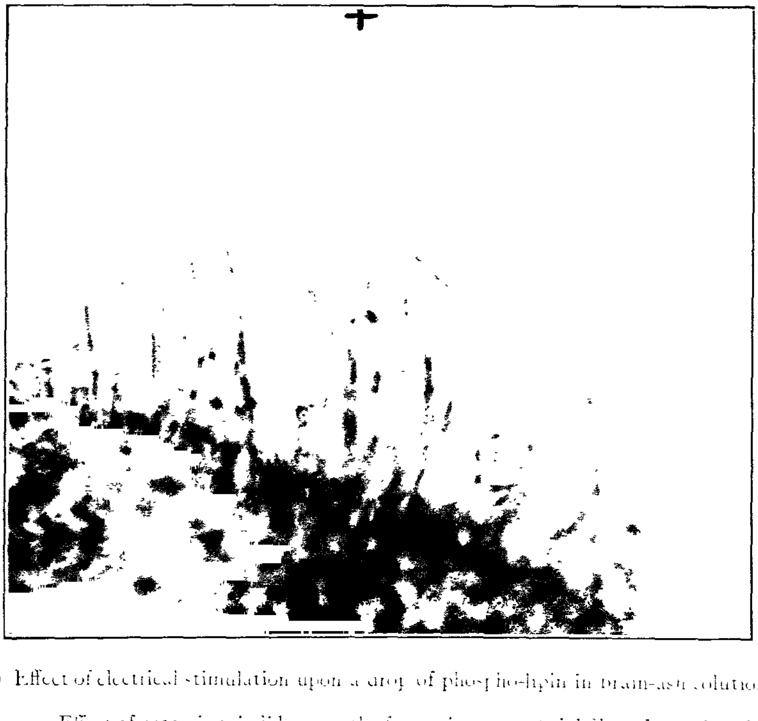
(*b*) Drop of phospho-lipin in a one per cent solution of morphine in brain-ash solution.

(*c*) Drop of phospho-lipin in a one per cent solution of urethane in brain-ash solution.

FIG. 3.—Effect of morphine and of urethane on the formation of artificial fibers from phospho-lipins of the brain. (From photomicrograph × 200.)



(*a*) Drop of phospho-lipin in a one per cent solution of KI brain-ash solution.



(*b*) Effect of electrical stimulation upon a drop of phospho-lipin in brain-ash solution

FIG. 4.—Effect of potassium iodide upon the formation of artificial fibers from phospho-lipins of the brain. (From photomicrograph × 200.)

the impact of the electric currents set up by the sense organs, the resultant energy waves or radiations produced by the swiftly moving electrons would result in a tapping as by trigger effect of the stores of energy in the innumerable cells of the nervous system. Such an action would explain the fact that electric stimulation of muscles and nerves is equivalent to normal brain stimulation, for they are identical.

We conceive that racial memory paths are laid down in fetal life by the nucleus of the ovum and of the spermatozoön.

This conception, although imperfect, harmonizes many known facts and fulfills many requirements. It requires just the material and only the material which is present in the brain as conductors and insulators arranged in systems. The possibility of infinitely small lines of conductance provides for millions of separate and distinct action or memory patterns. The semi-permeable quality of the cell membranes allows for association and for blending of memories—the white matter having essentially no oxidation is comparatively quiescent chemically and electrically, and being protected by a skull and a water bed, except for concussion, is free from physical disturbances. Its infinitely fine organization accounts for its capacity, and all of these together account for the permanency of the memory pattern, while at the same time the lability of a finely balanced electric system for receiving modifications is preserved.

As we have stated, the activity of our artificial brain models obeyed the Nernst formula for concentration cells. We conclude that memory apparently obeys the Nernst formula. That this is the case is suggested also by Fechner's law that the intensity of a sensation varies as the logarithm of the strength of the stimulus. This would be expected, since the mechanism for the production of a chemical secretion or the motion of a muscle is completely paralleled by the production of electricity by the brain cells. The production of electricity by the brain cells creates and motivates the action patterns, makes organs and glands do their specific

work, or causes local effects on other groups of brain cells, that is, produces memories.

This theory accounts for the more complex orientation of conductance and insulation into perfect conduction and memory by the repetition of stimuli. It readily interprets a massive orientation such as shell shock, produced by the powerful stimuli of life, and it would provide for the dramatic reëstablishment of normal action patterns by the specialist. It interprets the fact that every impression, whether for good or evil, is permanently written in the brain. It interprets education and social adjustments. It interprets the dependence of memory on bodily states, that is, on the electric potential. It accounts for changes in personality, such as are seen in the presence of hyperthyroidism, of myxedema, of cretinism. It accounts for the phenomena of the major emotions. It accounts for the temporary obliteration of the lesser memories of childhood by the stronger stimuli of later life, and on the other hand, it accounts for the reawakening of the memories of childhood as the stronger stimuli of life fade in old age. It provides a physical basis for the interpretation of reasoning and of invention. It brings memory under the dominion of the physical laws governing living matter and non-living matter.

SOME PHASES OF RAILROAD CONSOLIDATION

BY EMORY R. JOHNSON

(*Read April 24, 1930*)

RAILROAD consolidation is a perennial question that has been continuously before the country since the passage of the Transportation Act of 1920 authorizing railroad consolidations when approved by the commision and when worked out in accordance with the requirements set forth in the law. It is evident that consolidation was, in 1920, thought by the public to be desirable under conditions then existing. Those conditions were largely created by the World War and were in their nature, temporary.

The economic conditions of the country have greatly changed since 1920 and the railroads which were then to a large extent in a necessitous condition have, for the most part, become prosperous. Comparatively few railroads companies are now in a financially unstable condition. In all probability a bill containing the consolidation provisions to be found in the Act of 1920 would not become a law if presented Congress at the present time.

Does the country as a whole now want railroad consolidation? What are the views of the business and transportation interests of the country and what phases do those views give to railroad consolidation for the present and for the near future?

A conservative statement of the attitude of financial interests in the country towards railroad consolidation was contained in a publication issued by the National City Company of New York in March of this year. After setting forth the fact that the reason for country-wide consolidation that has been advocated in the past has to a great extent disappeared during the decade since the Act of 1920 was adopted, the statement is made that

"the final criterion should be the maintenance of existing routes and channels of trade so as to create as little disturbance as possible in existing business conditions in the public interest.
Railroad consolidation, in order to be carried out in the public interest, must be dealt with in a practical way. Dismemberment of well-articulated systems that are now functioning must not take place. Under the provisions of the Transportation Act consolidation must proceed on a voluntary basis, and, to accomplish this, consolidation must be sufficiently advantageous to afford an incentive to the carriers to go forward. In the existing circumstances, conditions must be accepted as they are and the best ajustment possible must be made that will afford a reasonable probability of attainment."

Probably most students of railroad consolidation would assume that the groupings of railroads that have been proposed by the interested railroad systems and by the Interstate Commerce Commission would work out to the advantage of New York as fully as they would work out with reference to any other port or section of the country. However, it is interesting to note that the Chamber of Commerce of the State of New York was sufficiently uncertain as to the advantages of railroad consolidation to the industries and trade of New York to have the subject investigated and reported upon by a transportation student of high standing, Professor T. W. Van Metre, of Columbia University. Professor Van Metre's report sets forth at some length his reasons for concluding that railroad consolidation would not be of advantage to New York and this conclusion is reached in spite of the fact that all of the four or five permanent eastern trunk lines systems would reach New York, as would also the consolidated railroad systems in New England. Professor Van Metre's statement is so definite and instructive that a quotation of a few of the significant paragraphs is justified. In his report he states that

"The outstanding feature of the Interstate Commerce Commission's complete plan of railroad consolidation, considered with reference to the railroad facilities of the Port of New York, is that the plan destroys the independence of the eastern trunk line railroads now having New York as their only seaport, and creates five trunk line

systems, each having terminals at two or more North Atlantic ports. Should this plan of consolidation be carried out, New York would not possess a single independent railway route to the West.
It is impossible to discern how these consolidations would result in any advantage to New York or to the railroads which now reach New York over their own rails. Not one of the proposed consolidations finds an excuse in the need of saving a weak railroad. The lines which serve New York now are in good condition physically and financially. Neither can the proposed consolidations be justified on the grounds of greater economy of operation and increased efficiency of service as far as New York is concerned. Those consolidations, if carried out, hold possibilities of grave injury to New York's trade and industry, injury which might eventually endanger the position which the city has so long occupied as the leading industrial, commercial and financial centre of the United States, and the chief gateway of the nation's export and import trade. For the commission's plan would deprive New York of an asset upon which, more than anything else, the prosperity of the port has been founded: a strong competitive advantage in transportation facilities. The plan would not only eliminate much of the interrailway competitive advantage in transportation facilities. The plan would not only eliminate much of the interrailway competition which has stimulated improvement in New York's transportation service; it would merge New York's hitherto independent railroad lines into systems serving the other North Atlantic ports and thereby weaken New York's position with respect to market and industrial competition."

Probably the most definite expression of the feeling that has arisen in the minds of at least a considerable part of the public is to be found in the joint resolution introduced into the Senate of the United States by Senator Couzens. The resolution was adopted by the Senate on the twenty-first of May this year, and has since been amended by the Committee on Interstate and Foreign Commerce of the House of Representatives. The resolution may not be adopted by the House but the fact that it passed the Senate with a substantial majority makes the statement contained in the resolution especially significant and expressive of public opinion. The resolution is preceded by several "whereases" which state in part that

"under existing law the Interstate Commerce Commission has not

adequate authority to protect the public interest in the matter of the consolidation or unification of railroad properties, and legislation conferring additional powers upon the commission has been under consideration by committees of Congress for several years; and that the Interstate Commerce Commission stated in its annual Report for the year 1929 that public regulatory policies would be defeated unless means were found to control holding company operations whereby railroad companies are now being consolidated, in effect, without any exercise of that public control which is necessary to conserve the public interest."

The Couzens' resolution was prompted by the opposition in the northwestern part of the United States to the consolidation of the Great Northern and the Northern Pacific Railroad systems. In its general plan of consolidation the Commission puts these two systems together. The preamble to the Senate resolution states that

"such a consolidation was held illegal by the Supreme Court in the Northern Securities case, and will substantially lessen and restrain competition in the Northwest from the Twin Cities to the Pacific Coast and it is charged, will seriously injure the growth and prosperity of many communities along the lines of these railroads."

The preamble also states that

"consolidations and unifications of railroad systems inevitably affect the prosperity of many communities along the lines of consolidating railroads and cause substantial changes in property values and result in losses of employment to railroad employees, which at the present time would add to agricultural and industrial depression and to extensive unemployment now existing among railroad employees."

Because of these and other reasons set forth in the preamble the Senate resolution provides

"That the authority of the Interstate Commerce Commission under existing law to approve and authorize any consolidation of railroad properties or any acquisition of control by one carrier by railroad of any other such carrier or carriers is hereby suspended until March 4, 1931, except in so far as such authority can and shall be exercised in conformity with"

certain requirements that are set forth in the resolution, the principal ones being that in authorizing consolidations the

Commission shall make such stipulations as will protect railroad employees from hardships or losses due to unemployment or shifting of residence; that any consolidation approved shall be one that would not be in violation of the Anti-Trust Laws and that the Commission shall not approve of any consolidation or unification effected through the agency of a holding company not subject to the jurisdiction of the Commission.

The relation of the holding company to railroad consolidation is being investigated by the House Committee on Interstate and Foreign Commerce. The investigation will be in progress for some months. There will probably be hearings this fall and the Committee will presumably submit a report to the House of Representatives either at the opening of Congress next December or shortly thereafter, recommending legislation for the regulation of such companies. The fact that the House Committee has this investigation under way makes it probable that the Committee will not favor the adoption at the present session of Congress of the Couzens' resolution, although it is not improbable that a majority of the House of Representatives would favor adopting some such resolution if no investigation of holding companies were under way.

If railroad consolidation is brought about it will undoubtedly be by a voluntary process and upon initiative of the carriers who will propose groupings to the Interstate Commerce Commission. The Commission will largely influence the actual grouping that takes place although it does not have the power to compel any carrier to acquire another carrier in the process of consolidation. Inasmuch as the process is to be a voluntary one, it is important to note what the policy of the carriers is as regards railroad consolidation.

The actual working out of railroad consolidations to establish large permanent systems having been held in suspense for a full decade, the rival railroad companies have naturally gotten into a frame of mind of opposing each other rather than of mutual coöperation. Their rival interests have been accentuated rather than lessened. The most active leaders

in efforts to bring about the enlargement of their systems are the Baltimore and Ohio, the Wabash, and the Nickel Plate-Chesapeake and Ohio systems in the east; the Great Northern and Northern Pacific systems in the northwest; and the St. Louis and Southwestern-Rock Island Companies in the southwest.

The Baltimore and Ohio is eager to acquire the Reading and Central of New Jersey and thus carry its large system through to New York. The Wabash desires to build up a fifth eastern trunk line group of railroads which shall take the system to New York by the Lehigh Valley (which is desired by the Pennsylavnia) and to Baltimore by way of the Pittsburgh and West Virginia and the Western Maryland. The Interstate Commerce Commission is also disposed to take the Norfolk and Western out of the control of the Pennsylvania Railroad and turn it over to the Wabash which will give that system entrance to Norfolk where connection will be made with the large Seaboard Air Line system extending to Florida.

The Nickel Plate and the Chesapeake and Ohio have already come together with the consent of the Interstate Commerce Commission. Under the able leadership of the Van Sweringen brothers of Cleveland, the Nickel Plate interests have obtained control of the large Missouri-Pacific system extending west and southwest from St. Louis. The Commission will allow the Nickel Plate to continue its present control of the Erie and has also included the Lackawanna within the enlarged Nickel Plate system.

As stated in the preamble to the Couzens' resolution there is popular opposition in the northwest to the merging of the Great Northern and the Northern Pacific. What the attitude of the public will be towards the proposed amalgamation of the St. Louis and Southwestern and Rock Island systems is not so certain.

The foregoing very incomplete statement in regard to certain railroad consolidations that are in the process of being worked out indicates that the New York Central is not vitally concerned in consolidation for the reason that its system is

fairly complete at the present time. The Pennsylvania Railroad System is generally considered by the public also to be not only as large and strong as, in the public interest, the system should be, but that it is adequately rounded out. This, however, is not the view of the officials of the Pennsylvania Railroad Company who have very definite ideas as to what should be done in connection with the accomplishment of railraod consolidations. The Pennsylvania interests having been left out of considerations by the other eastern trunk lines in proposals as to railroad groupings, and the declared needs of the Pennsylvania having been disregarded by the Interstate Commerce Commission in deciding upon its general plan of railroad consolidation, the Pennsylvania has adopted measures to protect itself.

Making use of the Pennsylvania Company, all of whose stock is owned by the Pennsylvania Railroad Company, the Pennsylvania interests have acquired almost half of the stock of the Wabash and by so doing have obtained a strong and probably financially dominant interest in the Lehigh Valley. By means of the Pennroad Corporation the Pennsylvania interests have acquired 74 per cent of the stock of the Pittsburgh and West Virginia which the Wabash will need as a part of a line to connect it with Baltimore. The Pennsylvania, mainly through the Pennroad Corporation has acquired substantial stock interests in the New Haven and the Boston and Maine which will be the two permanent New England systems. The Pennsylvania has long had a controlling financial interest in the Norfolk and Western.

The action of the Pennsylvania Company (controlled by the Pennsylvania Railroad Company) in acquiring potential control of the Wabash and a large influence in the Lehigh Valley has been challenged by the Interstate Commerce Commission as a presumptive violation of the Anti-Trust Laws. At the recent hearings of the Commission upon this question the President of the Pennsylvania Railroad stated clearly and frankly what the purposes of the Pennsylvania interests were in purchasing stock of the Wabash and Lehigh Valley roads. He stated that

"Out of that situation I expect to get one of three things, First, a large interest in a fifth system, or, second, a large interest in the Delaware and Hudson [from which the Wabash and Lehigh Valley stock was purchased] which is a valued and friendly connection or third, a large interest in the Lehigh Valley, which has been one of our objectives for many years."

In his testimony he also said

"In the purchase of the Wabash stock, I thought I would be safeguarding the Pennsylvania in a financial way and in every other way. I felt that I would have an investment for which, in the event of a fifth system, some one would have to pay roundly."

The Pennsylvania interests have good reason for desiring to bring the Lehigh Valley within their general system in order that they may have a direct line from the Great Lakes to New York and may also have additional trackage into New York City.

Enough has been said to indicate that the powerful railroad systems have clashing interests and that the major trunk lines are at the present time engaged in strategic maneuvers to protect their own interests and to checkmate their rivals.

The holding companies and the uses that are being made of them in bringing about the development of financial control of railroads present a problem that is important and may be difficult to solve. The extent to which the holding company is being used may be indicated by reference to the Pennsylvania and Nickel Plate systems.

The Pennsylvania Railroad Company has employed the Pennsylvania Company and the Pennroad Corporation to secure financial control of railroads whose acquisition are desired and which the Pennsylvania does not want other railroad companies to absorb. If the financial operations of the Pennsylvania Railroad Company as such were not subject to public regulation, without doubt the railroad company would have financed the purchases of the securities of other railroad companies. The purchase of the stock of the Wabash and Lehigh Valley was made not by the Pennsylvania Railroad Company but by the Pennsylvania Company because, as General Atterbury stated at the recent hearing

of the Interstate Commerce Commission (referred to above) in using that company he had only the Board of Directors with which to deal, that is to say, the transaction did not have to be approved by the Interstate Commerce Commission which would doubtless have refused its consent.

The Van Sweringen brothers have used in turn a surprisingly large number of investment or holding companies in carrying out their extensive plans of railroad consolidation. Among the companies that have been set up and made use of are the Vaness Corporation, the Special Investment Corporation, the Pere Marquette Corporation, the Allegheny Corporation, the Chesapeake Corporation and the Virginia Transportation Company. By employing these and other companies whose activities are not subject to regulation by the Interstate Commerce Commission a large measure of financial consolidation of railroads has been accomplished, a consolidation that promises to create, among others, the first really transcontinental railroad system.

The investigation and report by the House Committee on Interstate and Foreign Commerce will undoubtedly reveal the extent to which holding companies have been and are being employed in the development of the financial control of railroads. It is already evident that the regulation of railroad financiering by the Interstate Commerce Commission has been greatly limited by the use of holding companies in carrying out financial plans connected with railroad consolidations. It will be difficult, but probably not impossible, so to extend the jurisdiction of the Interstate Commerce Commission as to give it jurisdiction over the employment of holding companies in the financing of railroad consolidations, and to give the Commission this larger power in such a way as not to cause the government to interfere unduly with the financiering of industrial and commercial enterprises.

IDENTICAL TWINS AS BIOLOGICAL CONTROLS IN EDUCATIONAL AND OTHER HUMAN PROBLEMS

By ALBERT F. BLAKESLEE AND HOWARD J. BANKER

(*Read April 25, 1930*)

THE need of adequate controls in experiments can hardly be too strongly emphasized. It is generally agreed that experiments in biology are more difficult than in physics and chemistry. In the first place, it is generally more difficult to control the environmental factors to which the living organism is subjected. In the second place, peculiar difficulties arise in attempting to control the biological material. In higher forms of life at least, no two individuals are exactly alike, and the differences between individuals increase as one deals with increasingly complex organisms. In animals like man in which a high mental development has been reached, it is most difficult to secure proper biological controls.

Differences in human reactions are often discovered when least expected. An example may be mentioned from our own experience. In a culture of Verbenas, a certain pink-flowered plant and a red-flowered plant were accidentally discovered to be testers of human reactions. Twenty-six persons could perceive fragrance in the pink flowers but not in the red, while 13 found fragrance in the red but not in the pink flowers. Only two persons of the group tested found fragrance in both kind of flowers.

Comparable living material is as important to the biologist for accurate experimentation as is purified chemical material to the chemist. The student of heredity is increasingly realizing the desirability of purified strains of plants and animals in experiments, since when species in nature or commercial varieties are subjected to careful analysis they have been found to be made up of a mixture of diverse races or to be represented by individuals of different genetic constitution.

Many investigators without genetic experience, however, are satisfied with sunflowers grown from a packet of commercial seed or with guinea pigs purchased from the trade for their biological material, although they may be insistent upon the purity of their chemical reagents. Such too common disregard of the fact that the living reagents may differ in their physiological responses has often led to conflicting results of different investigators working with the same species.

When results are easily evaluated, over-refinement in an experiment may be a waste of time, but it is well for the experimenter to recognize the sources of error before making short cuts in his methods. It has seemed desirable, therefore, in the present communication to emphasize the importance of comparable biological controls as one of the desirable precautions in experiments with living organisms.

How may such comparable material be obtained? With higher plants it is a simpler problem than with higher animals. A single plant may be multiplied vegetatively, as by cuttings, and parts of only one plant used in the experiment. In most higher plants the two sexes are present in the same individual and by selfing for a few generations a relatively uniform race may be established. With higher animals which have biparental reproduction the problem is more difficult but by sister to brother matings continued for a number of generations, strains can be isolated incomparably better adapted to experimental purposes than the ordinary run of laboratory animals.

In man, the most interesting but probably the worst experimental animal, such purification is impractical. It is unfortunate that where controls are most needed it is most difficult to get comparable material for experiment. In human experiments, therefore, dependence is placed upon methods of random sampling and statistical treatment and conclusions are often drawn from data which, with forms better adapted to experimentation, would be considered inadequate. It should not be forgotten, however, that the mathematical reliability of conclusions bears no relation to the difficulty in securing adequate data.

The weak link in the chain of evidence in human problems appears to be the biological controls. As a method of strengthening this weak link we suggest the use of identical twins in human problems.

"Identical" twins are those which have come from the same fertilized egg in distinction from "fraternal" twins which have come from two different eggs. Fraternal twins may be of opposite sex and are no more alike than brothers and sisters of the same family. Identical twins are always of the same sex and strikingly alike in physical traits and mental reactions. Yet differences are sometimes found between identical twins probably due to slight differences in their environment during development. Furthermore, difficulties are sometimes encountered in distinguishing closely similar fraternal twins from identical twins. The fact remains, however, that in human problems the most comparable biological material is available in identical twins.

Is it feasible to employ such material in the study of human problems and would it be worth the extra trouble involved? We believe it is feasible. Whether such extra precautions are justified will depend upon the degree of accuracy required in the experiment and the importance of the problem.

As a concrete example of the use of what we believe to be the best possible biological material, we would suggest a school of identical twins for the study of educational problems. A new method of teaching could be tested with a class, each student of which had an identical twin in a control class, taught by the standard method.

Let us see if there are enough pupils available for such a school of identical twins. From data kindly furnished last October by Assistant Commissioner Geo. M. Wiley of the N. Y. State Educational Department, the number of pupils in N. Y. City in the upper grades and in the high schools were as follows:

5th grade—100839	1st yr. High School—101115
6th grade— 98023	2nd yr. High School— 54264
7th grade— 90167	3rd yr. High School— 28771
8th grade— 82298	4th yr. High School— 21756

This gives for the four upper grades a total of 371,327 pupils in Greater New York and 205,906 pupils in the high schools.

The ratio of twins to total births usually ranges in various countries from 1:80 to 1:90. The actual ratio in the Birth Registration Area of the United States as determined from Census Report for 1926 was found to be 1:88.5, but that for New York City was 1:101.6. In either case a high school population of 205,000 ought to yield over 2,000 pairs of twins unless we assume a heavy differential mortality against twins which doubtless is really the case.

The work of others has shown that about 25 per cent of twins born are identical and this proportion is quite generally accepted by special students of the subject. If that be approximately correct and we disregard any differential mortality among twins, for which we have no data, it follows that of the 2,000 pairs of twins calculated as in high school attendance there should be at least 500 pairs to be classified as identical twins. The same calculations would give over 700 pairs of identical twins in the four upper grades.

On account of the probably greater mortality of twins and other possible causes tending to reduce their relative number in school attendance, an attempt was made to obtain more reliable figures for the number of identical twins actually in the high schools in N. Y. City.

Through the kindness of Dr. J. L. Tildsley of the New York City Board of Education we received reports from 26 high schools regarding the number of twins in attendance. It was evident from these reports that complete data on the proportion of twins in the schools could not be readily secured without a special census taken for this purpose. To mention only a single difficulty involved;—a large proportion of the N. Y. City high schools are for boys or girls alone and when a pair of twins are bisexual the boy and the girl, therefore, are likely to be in different schools and hence often not recognized as twins by their teachers. This has brought it about that a pupil may be reported by one school as having a twin in another school which, however, does not record this pupil among the lists of twins submitted.

It is obvious, therefore, that a fair estimate of the number of identical twins in the N. Y. City high schools can not be obtained by the usual method of taking 25 per cent of the total number of twins listed. A fairer estimate probably can be obtained by the method employed by Newman which consists in taking 42 per cent of the cases in which both members of the pair are of the same sex. Since the unisexual twins which are not identical may show little resemblance one to the other and hence not be recognized as twins without a more careful search than was possible in the present instance, it is probable that this method of estimate will indicate a smaller number of identical twin pairs than actually exists. There were reported 188 unisexual pairs of twins from 26 N. Y. City high schools representing a student population of about 112,000. This sample is 54 per cent of the total high school population as given us last year. If we take 42 per cent of the 188 unisexual twin pairs, we obtain an estimate of 78 pairs of identical twins for about half of the high school population in N. Y. City. For various reasons, some of which have been already given, we believe this estimate below the number of identical twins actually in the City high schools. The estimate as it stands provides a supply of over 150 pairs of identical twins now in the N. Y. high schools which might be drawn upon to form an experimental school. A larger number would be available in the grades. It is evident from these calculations that there are at the present time a large enough number of identical twins in the schools of Greater New York to form such a school as we have suggested with thirty or more students per class.

It would be even more important, we believe, to have identical twin teachers than twin pupils for the test and control classes, since much of the success of a method in any given case depends upon the personality of the teacher. For identical twin teachers, one would have the whole country to draw upon and the supply, we believe, would be adequate.

It is not the place here to go into details regarding the organization of such a school. If an experimental school of identical twins is biologically a sound suggestion and would advance theory and practice in education, as we believe it would, there should not be lack of funds for its establishment.

RECENT TRENDS IN ANTHROPOLOGY

By FAY-COOPER COLE

(*Read April 26, 1930*)

In order to understand the present trends in American Anthropology, it is necessary to cite a few historical facts. If I were to tell the whole story I would be compelled to take you back to the anthropological observations and speculations of Lucretius and Herodotus. But the history of Anthropology as a science covers only a brief period.

In its early stages we find three types of interest manifest. The first dealt primarily with objects of material culture. In America the settlers of a century ago had actual dealings with the Indians or were living on soil which contained thousands of objects of their manufacture, and many gathered small collections. The interest thus stimulated led Lewis and Clark and other western explorers, military men and railroad builders to gather and send back materials, and also to make observations on native life and customs. Meanwhile European travelers, traders and missionaries were coming in contact with the less advanced peoples of the world and were obtaining many interesting examples of native craftsmanship.

Out of such collections museums ultimately developed. In America there had been several attempts to gather materials under one roof, but the first real museum was made possible by the gift of Smithson, which led to the founding of the Smithsonian Institution in 1846. We have not time to deal with the growth of similar organizations throughout the nation; the point of interest for the moment is that museums needed specimens, donors expected specimens, and this demand gave direction to the work of the staffs. Thus the developing science of Anthropology became objective, empirical.

The second interest we may call the theoretical. When

scholars studied the collections and stray bits of information brought home by travelers, they were quickly impressed with the many similarities in arts and crafts, and even social organization between people far removed from one another. Speculation on these similarities led to theories regarding the origin and development of culture, and eventually there emerged several well defined, logical explanations of human progress. This was just at the time when Darwin's Origin of Species was claiming the attention of the scientific world, and it is not strange that the theories sought to prove that man's society, like his body, had passed through certain definite steps or evolutionary stages. Even when more data was at hand, and when linguistic studies and collections of Folk Lore were available, the comparative and evolutionary viewpoints were dominant.

The third interest was in Physical Anthropology. The apparent differences between the various peoples of the world led to classifications and attempted explanations. Minute descriptions and measurements of living beings and of skeletons were taken in an effort to establish adequate criteria for the recognition of races. In America this interest followed a strange course. In the early part of the 19th century Phrenology was a respected discipline. It was then believed that faculties and functions of man were innate and that each had its definite region or location on the surface of the brain. Since it was an observable fact that individuals and races differed, it was self evident that there must be corresponding differences in the brain. It was also held that there was sufficient relationship between the outer surface of the brain and the skull so that a trained observer might recognize the centers by external examination. Such claims were long ago disproved, and Phrenology has fallen from the honorable place it once occupied. But interest in it led Morton to assemble the great collection of casts and skeletal material which is still to be seen in the Academy of Science in this city. This collection and the publications based on it indicated marked group and racial differences, and did a great deal towards

stimulating interest in Physical Anthropology. But here, as in Europe, there was a lack of thoroughly trained men, and most of the work was at best little more than a random sampling of a population.

During this early period no real archæological work was carried on in America. Some small exhibits gathered by farmers and local collectors did creep into the Museums, but there was no method or organization to the study. In Europe there was some interest in art objects of pottery, metal and stone, but the trite and unpretentious things which might have revealed the every day life of the makers were neglected. Chronology was but slightly developed and of little interest to most investigators. Needless to say, the application of anthropological studies to problems of every day life received scant attention.

Then came a period of protest. Intensive studies of a few cultures led to the recognition that world wide schemes, based on a few facts and much speculation, did not fit actual conditions. The supposedly sharply defined racial characteristics were found to be subject to great variation, while overlapping and race mixture made classifications of many individuals and even large groups exceedingly doubtful. It also became evident that there were regional differences in physical types as well as in culture.

Speculation based on inadequate data gave way to intensive studies, and for more than a quarter of a century the anthropological centers sought to develop only research workers who would investigate every phase of native life. A great mass of information was accumulated, many monographs published, but few generalizations or comparative studies appeared. During this stage the interests of American Anthropology were largely local, and its attack upon problems institutional. However, the founding of the Anthropological Association and the establishment of several anthropological journals gave promise of a new era.

The last few years have witnessed significant changes. Institutional jealousies have largely disappeared, and today

there is coöperation between the departments in various universities, museums and other research organizations. Last year the anthropological divisions of fifteen institutions united to found the Laboratory of Anthropology at Santa Fe, while frequent conferences have been held to discuss problems and methods.

Field work has been carried on with great vigor not only on this continent, but in Europe, Africa, Asia, and the Islands of the Pacific, and the findings have been correlated with investigations in other fields, both in the natural and social sciences. Here again we note a great change in attitude. A few years ago Anthropology was largely isolated. Today it is actively participating in the National Research Council, the Social Science Research Council, the Learned Societies, and many local research organizations, and is closely associated with those disciplines whose studies bear on the origin and development of man and his culture. These contacts have been of great value to Anthropology. We have borrowed liberally from Geology, Paleontology, Geography, and History. We have adapted their procedures to our problems, and have thus been enabled to establish sound methods for studying culture growth, culture sequences, and their spatial distribution. We likewise recognize our debt to the Biological Sciences, and our students in Physical Anthropology are required to have full training in Physiology, Embryology, Anatomy, Pathology and Genetics. They still study races and race mixture, but today interest centers on the families and groups which make up the larger divisions. In place of random sampling they attempt, so far as possible, to substitute laboratory conditions. They investigate problems of growth, of race mixture, and the effects of environment on known populations. The studies of Boas on the Descendants of Immigrants, of Hrdlicka on Old Americans, and of Todd on Growth, are a few among many which are of utmost importance to all who are interested in the racial type and physical welfare of the American nation.

The Anthropologist is also contributing to the study of

Eugenics, but he insists that before we are ready to legislate we must have all the facts before us. We must apply the findings of both the physical and social sciences in order that we may be able to determine the relative importance of cultural experience and racial descent. We must know which traits are hereditary and which may be affected by the environment. For example: It has been customary to use stature, head and face form as the most fixed of racial characters. Classifications have also been based on language; while recent books by well-known writers claim that achievement is a proof of superiority. Let us weigh these claims for just a moment.

On the basis of repeated measurements we now know that not only stature but head and face form can be affected by the environment. We now know that the language we speak and the way we classify our sense experiences may be modified by contacts. A survey of man's long past, as revealed by Archæology, shows us that a physically fine and potentially great people like the Cro-Magnon will remain on a low cultural level if left in isolation. It likewise shows us that the lead in culture and civilization has shifted from one people and region to another—from the Egyptian of 2000 B.C. to the Greek of 500 B.C., to the Roman of the beginning of our era, and finally to the northern barbarians. With such evidence before us it is evident that we need facts, not theories.

The anthropological student in linguistics has joined with the Psychologist in bringing new light to bear on the mental processes of man. Ethnologists and archæologists have reached a stage where they can analyze and compare their findings and can draw conclusions from observed facts. This has resulted in a considerable number of general books which are available to the related fields.

Anthropology has profited greatly by its contacts, and it has likewise been able to offer much to its sister sciences. Its value in furnishing a world-wide view of man and his society is now recognized by many American Universities, and new departments are being established each year.

Not the least important in the present trends is the dis-

position of Anthropology to extend its investigations to the culture of our immigrant peoples. The techniques developed in our studies of simpler cultures are now being modified and applied to our alien groups. So long as we drew our population largely from northern Europe there was little difficulty in adjusting the newcomers to American conditions, but with the influx from southern Europe the situation changed. We were then forced to deal with people whose social, economic and mental backgrounds were very different from our own, and our attempts to incorporate them into our national life were far from successful. To remedy this situation Anthropology is carrying on intensive investigations of these people in their home-lands. It is studying the physical types and cultures of the peasants of Sicily, Mexico and other regions from which we have drawn or are still drawing large numbers of immigrants, in order that we may be able to intelligently direct their adaptation to American life and conditions. The practical application of such studies is beyond question. If you wish to understand the Mexican in Chicago you must first know him in the home-land.

We can then summarize the present trends in Anthropology by saying that coöperation between its own workers is actually established, and its relations to the sister sciences are becoming daily more intimate. That it has largely divorced itself from theory and is seeking to solve the problems of race and civilization by intensive studies. Nevertheless it is now beginning to generalize its findings and is thus making them available to a larger audience, with the result that it is rapidly taking its place in American University life. Finally it is interesting itself in the solution of present day problems. Through its objective point of view with reference to all cultures it is furnishing valuable data for those who have to deal with our alien problems and with international affairs.

UNIVERSITY OF CHICAGO

ON CERTAIN SILURIAN FISH-LIKE FORMS FROM NORWAY

By WILLIAM BERRYMAN SCOTT

(*Read April 24, 1930*)

ALMOST exactly fifty years ago, in June 1880, I presented to the Philosophical Faculty of the University of Heidelberg a dissertation on the *Embryology of the Petromyzontidæ* (Lamprey Eels). The lampreys are universally recognized to be far below the true fishes in grade of organization, but how that inferiority is to be interpreted, is a very difficult problem. How far it is due to primitiveness of structure and how far to degeneration, is a question as to which zoölogists have differed and still continue to differ. It was hoped that the remarkable embryological material which Dr. E. C. Calberla had spent four years in collecting and preparing, and which was handed over to me by Professor Gegenbaur after Dr. Calberla's death, would throw much light upon these problems concerning the true status of the lampreys (Cyclostomata). Let me remind you, as a preliminary, of the classification of the existing and some extinct Vertebrata.

VERTEBRATA

A. ACRANIA

B. CRANIOTA

I. MONORHINA
   1. †ANASPIDA
   2. †CEPHALASPIDA
   3. CYCLOSTOMATA

II. DIPLORHINA (GNATHOSTOMATA)
   1. PISCES
   2. AMPHIBIA
   3. REPTILIA
   4. AVES
   5. MAMMALIA

† Extinct.

The term Cyclostomata is meant to indicate the circular, sucking mouth, set with rows of horny teeth, which is devoid of jaws, or true teeth; while the Gnathostomata, or animals with jaws, include all vertebrates above the rank of the lampreys. Another unique feature of the Cyclostomata is the single, unpaired olfactory organ, which opens in the middle line on the top of the head. Behind this is visible, through the skin, the pineal gland, which in the existing lizards remains what it almost certainly originally was, a functional third eye. In other vertebrates the pineal gland has been diverted to different uses and is, as its name applies, one of the ductless glands. The gills are arranged in pouches, which open externally by means of small, oval gill-slits, and it is these pouched gills from which is taken the name, Marsipobranchii, often used instead of Cyclostomata. The number of slits and pouches is from seven to fourteen pairs in the different representatives of the class. The lampreys have a naked, slimy skin, without trace of scales, and there are no paired fins, but there is a cartilaginous skull and an elaborate branchial basket, also of cartilage.

The embryological development is much complicated and obscured by the existence of a larval stage, so very unlike the adult, that the larva was long regarded as belonging to a different genus and given the name *Ammocœtes*. This strange creature lives buried in sand or mud in total darkness, and its higher sense-organs are in a state of arrested development. The minute and unfinished eye is covered over with masses of muscle, so that it cannot be of any functional use. The olfactory organ remains very simple in comparison with its remarkable and elaborate complexity in the adult. The mouth has a prominent, semicircular lip, entirely different from the sucking disc of the adult, and there are no horny teeth.

The study of lamprey embryology led to some quite definite results, but, at the same time, was ambiguous as to many other characteristic features; and, when I had completed my work, it still remained an open question whether the peculiarities of the lampreys were primitive, or due to degeneration, or whether both factors might not be involved. The absence

of scales might be interpreted as due to the loss of these structures, as is undoubtedly true of the eels, properly so called, the cat-fishes and others, or the naked skin might be a primitive feature, scales never having been formed in the lampreys or their ancestors. The same considerations apply to the absence of true teeth, of jaws, and of paired fins. The alternatives are thus stated by President Jordan:[1]

"From the true fishes the Cyclostomes differ in the total absence of limbs and of shoulder and pelvic girdles, as well as of jaws. It has been thought by some writers that the limbs were ancestrally present and lost through degeneration, as in the eels. Dr. Ayres, following Huxley, finds evidence of the ancestral existence of a lower jaw. The majority of observers, however, regard the absence of limbs and jaws in Cyclostomes as a primitive character, although numerous other features of the modern hagfish and lamprey may have resulted from degeneration. There is no clear evidence that the class of Cyclostomes, as now known to us, has any great antiquity, and its members may be all degenerate offshoots from types of greater complexity of structure."

This last suggestion of wholesale degeneration may now be definitely set aside; the combined testimony of embryology and palæontology suffices to make, at least, a first approximation toward a solution of the Cyclostome problem, the remarkably well-preserved fish-like †Anaspida, discovered and described by Kiaer, supplementing the inferences from ontogeny in a truly surprising way.

A very remarkable and unexpected feature of the newly hatched larva (a little creature hardly more than 1/8 inch long) is in the character and shape of the mouth, which has not the slightest resemblance to the sucking disc of the adult. In this stage of development, the mouth is a transverse slit on the ventral side of the head, relatively a considerable distance behind the anterior end of the snout. In form and position this mouth is surprisingly like that of a miniature shark. The olfactory organ, also, which is single and placed in the median line from its first inception, is, in this stage, on the ventral side of the head, and forms a small, triangular

[1] *Guide to the Study of Fishes*, Vol. I, p. 487.

depression in front of the mouth. This is in strange contrast to the adult condition, in which the nasal opening is on top of the head.

The most important factor in bringing about the characteristic changes in the development of the head is the formation of the mouth, even in the imperfectly suctorial mouth of the larva, *Ammocœtes*. The upper, or rather the anterior, lip begins to elongate and rotate, passing through an angle of 180°, so that its originally posterior margin now points directly forward. This rotation completely changes the shape of the mouth and, at the same time, brings the nasal opening to the top of the head. The pineal gland (epiphysis) shows very distinct evidence of having once been a third eye, as it still is among the lizards, the only existing group of vertebrates in which this eye is completely formed and functional. In the embryo lamprey no lens is developed, but there is an obvious retina, likewise an optic stalk. In no other existing vertebrates, except the lizards, is the optical nature of the epiphysis so distinctly shown, as it is in the lampreys.

The fish-like animals (†Anaspida and †Cephalaspida) of the Downtonian division of the Silurian period were first discovered by Dr. Traquair in Scotland, but his material was, for the most part, so incomplete that it was misinterpreted. Dr. Kiaer has been very much more fortunate in securing a large amount of material in a remarkably fine state of preservation; and this newly discovered material renders obsolete the statement of Jordan, entirely accurate when it was made, that "there is no clear evidence that the class of Cyclostomes, as now known to us, has any great antiquity." Of these Silurian †Anaspida, one genus †*Lasianus*, has but feebly developed dermal skeleton, but scales are present and the line of dorsal spines is very conspicuous. In the other genera, the body and head are completely covered with scales, which make a continuous armour of closely fitting plates. While there is no likelihood that the †Anaspida were the direct ancestors of the lampreys, there is strong reason to believe that they were nearly allied to those ancestors, and this

suggests that the naked skin of existing Cyclostomes has been acquired through the loss of an original covering of scales.

The †Anaspida have seven pairs of gill pouches, the openings of which are circular in outline and, most important of all, they have a single olfactory organ which opens on the top of the head. The pineal gland had an opening through the roof of the skull, though it is, of course, impossible to determine whether it was a functional eye. There is no internal skeleton; at least, no trace of it has been found, though there must have been some kind of internal support, which was presumably cartilaginous. It is not known, therefore, whether these creatures had jaws; but the arrangement of scales around the mouth to form a sort of beak, is, perhaps, an indication that true jaws were not present. The mouth is terminal in position, and equally unlike the sucking disc of the lamprey, and the transverse, ventral slit of the sharks. There were certainly no teeth, for these would have been indicated in such well-preserved specimens.

Most interesting of all, are the indications of paired fins, of which only the anterior, or pectoral, pair are present, if indeed those structures are really homologous with pectoral fins. If so, they were not functional as fins, but formed merely an incipient stage, being composed only of modified scales and spines and having no internal skeletal elements, such as make up more or less of the pectoral fins of all true fishes. In †*Lasianus*, which is the most primitive of the †Anaspida, there are, on each side and behind the gill-openings, eight spines, which in the more specialized genera are reduced to a single, much larger, pointed spine.

These remarkable Norwegian fossils do, I think, actually clear up the problem concerning the origin and systematic position of the lamprey group. They show that the group is one of great antiquity, that in association with a generally primitive condition, they display certain remarkable specializations and other changes which may or may not be regarded as due to degeneration; that is largely a question of definition.

The unpaired olfactory organ, the pouched gills, the ab-

sence of jaws and true teeth and of paired fins, are primitive characteristics, which date back at least to the Silurian. The sucking, disc-like mouth, set with rows of horny teeth, is a specialization which no other vertebrates have acquired and, as the embryology shows, it is the development of this exceptional mouth which so modifies and disguises the ontogeny. The loss of scales can hardly be called degeneration, though it is sometimes so regarded, but this is more a matter of words than of facts. At all events, it is a sufficiently strange coincidence that the problems left unsolved by an embryological investigation, should find their solution more than forty years later from the fossils of Norway.

AN INTRODUCTORY STATEMENT TO THE SYMPOSIUM

By JOHN A. MILLER

(*Read April 26, 1930*)

THE Committee in charge of the program for this annual meeting of the *American Philosophical Society*, believing that an integration of the results of recent researches in one or more fields of human endeavor is one of the functions of a society founded for the Promotion of Human Knowledge, appointed a sub-committee to arrange a colloquium to do this integration for astronomy and astrophysics. I am speaking for this sub-committee.

We found that the assignment was too broad, that it would be impossible to give even an incomplete survey of the accomplishments of a decade. We chose instead, with the consent of the General Committee, to present for the consideration of members of the society, a somewhat exhaustive treatment of one subject in each of four fields, viz., the Solar System, the Galaxy, the Interplay of Physics and Astronomy, and Recent Contributions to our knowledge of Super-galaxies.

For the Solar system we chose that problem which seemed to us fraught most heavily with significant consequences, and have asked that astronomer to present it, who can speak most authoritatively about it.

This does not mean that the Committee overlooked the significance of other researches in this well worked field, such for example, as a more intimate acquaintance with our planetary atmosphere; or the increase in our knowledge of planetary radiation; or the contributions to theoretical physics resulting from studies of the sun.

Because of the attention recently given to the body, Planet *X*, a reference to it might not seem impertinent. The announcement of the discovery of the body was made after this

program was formulated, and it has been observed many times since then. Orbits have been computed at the Lowell Observatory and the University of California. However, since the arc of the orbit described by the planet since its discovery is very small—less than one quarter of a degree—it is practically certain that these computed paths will differ, perhaps radically in certain essential particulars, from the one chosen by the planet itself. It is very probable, however, that the inclintion of the plane of its orbit is about seventeen degrees—larger than that of any of the major planets. Its distance from the sun, at present forty-one astronomical units, is greater than that of any hitherto known planet, and the eccentricity of its orbit is almost certainly much larger than that of any of the major planets. That is, it is probable that this body moves in an orbit very different from that that tradition has led us to believe a well-behaved planet would choose. On the other hand, the last discovered satellites of Jupiter and Saturn have, in the same particulars, violated the traditional rules of the orbits of satellites. Previous to that the Jupiter system had been considered a miniature replica of the solar system. But the discovery of these satellites with very eccentric orbits and orbits greatly inclined to the orbit of Jupiter destroyed this analogy. If the computed orbits of Planet *X* are even approximately correct the analogy would be in a part restored. Just as the outer satellites of Jupiter multiplied many times the dimensions of that system, so if the new body is a planet, the dimensions of the solar system have been at one stroke multiplied many times.

The Dominion Observatory of Ottawa has just announced the discovery of another body, also suspected to be trans-Neptunian.

Leaving the solar system we chose a problem in our galaxy, farther away from our immediate neighborhood, it is true, but still in the field of our domestic relations. Our researches in the galaxy in the last decade or two have been much of the Durch-Musterung sort. For example, influenced by Professor Schlesinger, more than any other one, we have literally found

very accurate distances of thousands of stars from the sun. New methods for determining distances have been devised. With the spectroscope we have literally found the motions of thousands of stars. In a certain sense, the second paper of this symposium is an integration of these phenomena. But, in choosing this subject, we, as in the case of the solar system, have not overlooked the stupendous importance of the great mass of contributions that have been made in many other directions. For example, by spectroscopic analysis we have learned of the chemical composition, temperatures, the state of energization, and the masses of stars. Extensive photometric surveys have been made. Different phenomena have been correlated and I believe it safe to assert that in no previous period of American astronomy have statistical methods been applied so frequently or yielded such brilliant results.

Our third paper is an account of the intimate interplay between physics and astronomy.

But all these have to do with domestic problems. The fourth paper of the symposium leads us abroad, outside the little continent of space occupied by our galaxy, to other galaxies, perhaps to a galaxy of galaxies. Alexander Pope, Whitman, and others dreamed of their existence, but it seems to have been left to our generation to give a physical proof of their existence.

THE STRUCTURE AND ROTATION OF THE GALAXY

By J. S. PLASKETT

(*Read April 26, 1930*)

Our conceptions of the magnitude and complexity of the galaxy or stellar system have grown with increasing knowledge of the distances of the stars. The idea of the disc-like form of the stellar system, almost obvious to naked eye observations, was introduced by Wright about two hundred years ago, was later elaborated by the elder Herschel and has been confirmed by all more recent investigations. The early guesses of the 19th century at the dimensions of this disc or watch-shaped system were based on meagre and uncertain data and it was not until 1905 that Newcomb and Seeliger, who were the first to apply scientific methods to the problem, gave an estimate of 7,000 light years for the diameter of the stellar system. This was increased by Walkey in 1914 to 10,000 and shortly afterwards by Eddington from theoretical considerations to 15,000 light years. The most complete investigations of the structure and dimensions of the watch-shaped stellar system is due to Kapteyn who in 1920, after extended investigations on the distribution of the stars, practically his life work, gave a graphic representation of the structure of the stellar system. In his picture, as can be seen from the diagram, the stars thin out gradually, with increasing distance from the sun, although more slowly along the direction of the Milky Way. The diameter of this hypothetical system, where the stars are only one tenth as closely spaced as around the sun, is about 18,000 with a thickness of 3,500 light years while the corresponding dimensions, where the star density is only one hundredth, are 55,000 and 11,000 light years respectively.

But all these estimates referred to a simplified ideal watch-

shaped system of stars surrounding the sun, which was placed approximately at the centre of this system, with the stars gradually thinning out with increasing distance from the sun. Even the most cursory glance at the Milky Way on a clear, dark night suffices to show how far from uniform is its structure. It seems to be formed of great aggregations of star clouds separated by sparser regions and by dark markings and is very far from the regularity pictured by Kapteyn and others. This is emphasized by an inspection of photographs of some of the Milky Way clouds and no one who sees this irregularity in the distribution of the stars can help but realize that the ideas of the stellar system just described were but simplified representations of a much more complex structure. While they probably well represent the arrangement of the stars in the neighbourhood of the sun, the local cluster as it is now generally called, they take no account of the irregular distribution of the stars around the Milky Way into the great star clouds we have just seen.

An entirely new conception of the magnitude and complexity of the stellar system or galaxy was given by the researches of Shapley at the Mt. Wilson Observatory some twelve years ago. The period-luminosity relation of the Cepheids developed by him gave him a powerful method of determining stellar distances which applied to the globular clusters showed them to be from 20,000 to 200,000 light years from the sun. When their positions were plotted with respect to the central plane of the Milky Way, the remarkable and important fact was established that they were symmetrically distributed with respect to the plane with the same numbers on each side of it. Such a distribution could scarcely be accidental and there seemed no escape from the conclusion that these 80 odd globular clusters were numbers of the galactic system which was probably concentric and coterminous with them and was hence a much larger and more complex organization than previously supposed. The central position of the sun was irretrievably lost when the distribution of the cluster indicated it to be about 60,000 light years from the centre of this

great system which Shapley estimated as some 300,000 light years in diameter and 20,000 in thickness.

Although more recent work has indicated that the distances of the clusters and the scale of the system may have to be somewhat reduced, possibly by 30 per cent, even this reduction gives a system 200,000 light years in diameter, large enough surely for the most pronounced anthropocentric leanings.

The modern conception of the stellar system is then not a single watch-shaped cluster of stars surrounding the sun and gradually thinning out towards the edges but rather a great aggregation composed of this cluster and numerous others represented by the Milky Way clouds. These clusters or clouds gradually merge into one another at the edges where the stars are thinner, the whole forming one great disc-like system approximately circular in outline with its thickness only about one twentieth of its diameter. The globular clusters, however, are nowhere in this disc but have a roughly spherical or ellipsoidal form surrounding and concentric with the disc and are symmetrically distributed with respect to it as indicated in the imaginative conception of the galaxy on the screen. They are, however, only of secondary importance compared to the main disc-like system which has been estimated to contain at least 30,000,000,000 stars.

Of the actual structure of this main system we have little definite direct evidence except that it must be composed of a great collection of star clouds of disc-like form. It has been supposed that it has a general resemblance to the great spirals whose distances and dimensions have been determined by Hubble by the same method as Shapley used for the clusters. Although they are all considerably smaller than the galaxy, the largest of them, the Andromeda Nebula being about 45,000 light years in diameter, only one sixth or so of the galaxy, their dimensions are of the same order, while the greatly flattened shape is common to all. Moreover, they all possess a regular structure of the same general form though with variations in detail as can be seen from the examples. A strong central

condensation from which apparently unwind arms of spiral form with secondary condensations of varying magnitude is characteristic of all the spirals. This, with the extremely flattened shape, almost inevitably suggests that they must be in relatively rapid rotation and this has been confirmed in some cases by spectroscopic observations. When our stellar system is also in the form of a thin disc and if, as we shall see later also from spectroscopic evidence, the internal motions suggest an orderly and majestic rotation of this great system, we can hardly escape the conclusion that our own galaxy is a great spiral nebula and that the local cluster and the great star clouds are secondary condensations in spiral arms in this nebula.

This conclusion and our inherent belief in the uniform structure of all the galaxies is confirmed by photographs on a large scale by the 100 inch telescope of parts of the great spirals in Triangulum and Andromeda. No one who compares the star clouds in these outside systems, especially when the difference in scale is considered, with those in our own galaxy can fail to be convinced of the similarity in structural details and to be impressed by this additional evidence of the essential homogeneity of the universe.

This introduction should have enabled us to form a general idea of the structure of the galactic system and indicated the probability of rotational motion in such a flattened system. Considerable speculation during the nineteenth century on the rotation of the stellar system around some central supreme body, our sun being naturally selected by one school and Alcyone by another as the centre of the universe, was however, based on very uncertain evidence and could not survive so that little has been heard for several decades of a rotation of the galaxy. The question was, however, placed on an entirely different basis about four years ago by the Swedish astronomer Lindblad who assumed that the great galactic system, constituted as we have attempted to describe, was composed of a number of sub-systems approximately concentric with one another and all in rotation though at greatly different speeds

about a common axis perpendicular to the galactic plane. The sub-system with the highest speed of rotation, and consequently with the smallest peculiar or random motions, would by centrifugal force be the most flattened towards the central plane and is, on Lindblad's hypothesis, the one containing our sun, the local cluster, the Milky Way clouds and the vast majority of the stars. Sub-systems with a lower rotational speed and hence higher random motions will be less flattened to the plane and the stars in these systems, on account of our high rotational speed estimated as 300 km. per second will appear to be moving much faster, hence the name "high velocity" stars, than the stars in our own system. The most slowly rotating sub-system was assumed to be the system of globular clusters which are little flattened to the plane, have high peculiar velocities and by reason of the high rotational speed of the sun of 300 km. per second, should appear to and do have velocities of this order in the line of sight.

Lindblad's theory was immediately successful in explaining some puzzling systematic motions of the stars. Stromberg of Mt. Wilson some years earlier had directed attention to a curious systematic trend in the radial motions of the 'high velocity' stars, of the globular clusters and of the spiral nebulæ which all appeared to have a preference of motion towards one direction in space, which was nearly in the galactic plane and towards galactic longitude 61°. Stromberg offered no satisfactory explanation of this phenomenon which he called an "asymmetry in stellar velocities" but it follows directly and simply from Lindblad's theory. The direction of asymmetry is nearly at right angles, 94° to be exact, to the direction to the centre of the globular cluster system, which Shapley has recently calculated as galactic longitude 327°, and is evidently an apparent motion produced by the high rotational velocity of the sun at right angles to the above direction, to the centre of the galaxy at 327°. Lindblad's theory also offers a fairly satisfactory explanation of the phenomenon of star streaming and was hence immediately successful in explaining these hitherto mysterious systematic motions of the stars. He did

not, however, bring forward any direct observational evidence of a rotation of the galaxy but this was later supplied by a young Dutch astronomer, J. H. Oort of Leiden who in 1927 developed the mathematical consequences of Lindblad's theory and applied them to the known motions of the stars.

Before discussing Oort's analytical work, it will be well to consider the general consequences of a rotation of the galaxy. Obviously it is by no means an easy problem to detect a rotation of the galaxy from our position within the rotating system itself, where all the stars of the system we can observe also partake of the rotation. For any rotation there must be some central attracting force, which is obviously provided by the matter of the stars themselves and is equally obviously directed towards the geometrical centre. This central force must be more than sufficient to make the orbits of the stars closed to keep the system together, else it would soon dissipate into external space. The law of force will depend upon the distribution of the stars within the system and two particular cases may be considered, that of uniform distribution and that of a condensation towards the centre.

In the first case, that of uniform distribution of the stars or matter throughout the system, elementary dynamics at once tells us that the attractive force will be directly proportional to the distance from the centre and that all the stars and other bodies of the system will have the same angular velocity, they will all revolve around the centre in the same time or the system will revolve like a solid disc or wheel. For uniform distribution of the stars throughout the system, there can be no relative motions between the stars and hence no rotation can be detected from observations of the radial velocities or proper motions of neighbouring stars. Just as a fly on the spoke of a rapidly rotating wheel could not tell by observing other flies on the rim, hub or spokes whether the wheel was stationary or spinning.

The position is more hopeful in the second case, that of condensation of the stars or matter towards the centre. If the galactic system is considered as structurally similar to the

spirals, it seems highly probable from the photographs of these objects that there is such a central condensation. In the extreme case where all the matter is concentrated at the centre, practically true of the solar system, the attracting force is inversely as the square of the distance and the bodies or stars nearer the centre revolve faster, have higher linear and angular velocities, than those further out. In such a case there is relative motion between neighbouring bodies or stars in the system and a rotation of the galaxy could be detected and measured from the differential motions thereby imparted to neighbouring stars. All the matter of the stellar system is not, however, concentrated at the centre and the law of force will be some function of the distance intermediate between the direct and the inverse square. Similarly the revolution will follow some intermediate course between constant angular velocity and planetary motion but it is obvious that as long as there is any condensation towards the centre there will be relative motion between the neighbouring stars which can be measured and the rotation determined.

Oort's great contribution to the problem of the rotation of the galaxy, which appeared in a masterly series of papers in the Bulletin of the Astronomical Institute of the Netherlands in 1927 and later, was to translate the above general principles into mathematical language and test the resulting expressions from the known motions of the stars.

The formula, on the assumption that the distance to the centre of the system is very large compared with the distances of the stars observed, takes the following form for radial motions:

$\overline{V} = V_0 \cos \overline{D} + \bar{r} A \sin 2(\bar{l} - l_0) \cos^2 \bar{b} + K$ where

$\overline{V}$ = Average radial velocity for a limited group of stars

V_0 = The solar velocity

$\overline{D}$ = Average angular distance between the group and the solar apex

$\bar{r}$ = Average distance of the group of stars from the sun in parsecs

$\bar{l}, \bar{b}$ = Galactic coördinates of the group

l_0 = Galactic longitude of the gravitational centre
K = Average residual velocity with regard to sign — the K term
$A = \frac{V}{4R}\left(1 - \frac{R}{k}\frac{\partial k}{\partial R}\right)$ = Rotational radial effect in km. per sec. per parsec
$B = A - (V/R)$ = Transverse term in km. per sec. per parsec
R = Distance from the sun to the centre
k = Total gravitational force
V = Circular velocity in km. per sec.

There is a similar formula for the proper motions but as only radial velocities will be discussed it is unnecessary to complicate matters by giving it. The above formula may be simplified by neglecting the $\cos^2 \bar{b}$ term for stars near the galaxy and using the residual radial velocities, $\bar{\rho}$, the velocities after the removal of the usual solar motion. The formula then takes the simple form

$$\bar{\rho} = K + \bar{r}A \sin 2(\bar{l} - l_0)$$

and it is at once seen that the rotational term varies with the distance and with the sine of twice the angle between the star or group of stars and the direction to the centre, it has a double wave form as contrasted with the single wave solar motion. A graphical diagram may help those who prefer geometrical representations and it can be at once seen by either method that the rotational effect will be zero along and perpendicular to the direction to the gravitational centre and alternate from positive to negative maxima at intermediate positions.

Oort showed conclusively, from an analysis of the known radial velocities of all the more distant celestial objects such as the O, B and N type stars, the Cepheids and the "c" stars, the planetary nebulæ and the calcium clouds, that all these objects showed distinctly a double wave swing, which if not due to a rotation of the galaxy, must be produced by some cause which gives identical results to galactic rotation. The number of distant objects available to Oort was, however,

small and the probable errors of the rotational term relatively high. It would seem desirable therefore if more observational evidence, more radial velocities of distant stars, were available for the discussion, especially when the effect of random velocities on the mean velocities of small groups of stars is considered.

Fortunately at Victoria, J. A. Pearce and the writer had been observing since 1924 the radial velocities of all BO to B5 stars brighter than 7.5 visual magnitude and north of declination $-11°$ which had not been previously investigated. There were about 450 stars in this programme and as they are faint and the B stars are of high luminosity, they are relatively distant and especially useful for testing the rotation of the galaxy. A preliminary paper in Monthly Notices discussing part of these velocities gave a strong confirmation of the reality of the galactic rotation but as all the velocities have since been completed, it will be preferable to give illustrations from the final material. Altogether with these new radial velocities of 450 BO to B5 stars, there were available the radial velocities of 995 O to B5 stars. After the exclusion of stars for which the velocity is uncertain, of stars wrongly classified in the Henry Draper catalogue and of stars whose residual radial velocities were abnormally high, the limit being generally taken as 40 km. per sec. about four times the random or peculiar velocity of the B type stars, there remained for discussion the radial velocities of 875 O to B5 stars.

Considerable judgment and experience are necessary to arrange these to the best advantage and before giving any results or comparisons some description of the methods of grouping and solution is desirable. It has already been found that the rotational term is directly proportional to the distance of the stars, or groups of stars, from the sun and the first requisite, therefore, is to arrange the stars into groups at different distances, else the solutions will give only rounded results. This is, however, a difficult problem for the very distant O to B type stars where no reliable individual parallaxes are available. As a first step, since it is certain that the O to B2 stars are con-

siderably more luminous than those of types B3 and B5, the stars were arranged into these two main groups. As a first approximation, assuming small dispersion in the intrinsic luminosity or absolute magnitudes of the stars, they were arranged in order of distance by dividing into groups in apparent magnitude in two ways as shown in Table I, the first giving four and the second eight groups presumably at different distances.

But the rotational term also varies with the sine of twice the angle between the stars and the direction to the centre. It would be possible of course to solve these 12 distance groups by least squares using the stars individually but this is inadvisable not only because of the labour involved but also on account of the random or peculiar motions which for the O and B stars are of the order of 10 or 12 km. per second and, of course, often reach much higher values. The effect of these random motions can be partially removed by combining the stars into groups and consequently in all the solutions the distance groups were arranged into sub-groups in longitude. As the rotational effect is a function of twice the angle, these sub-groups ought not generally to extend over a much greater longitude interval than 20°. This frequently limits them to two or three stars, insufficient to compensate for large or irregular random motions but this cannot be avoided with the limited numbers available.

The results of the least square solutions for the first arrangement into four groups, two magnitude divisions, which was carried through for $\bar{r}A$, l_0 and K are given in the upper part of the table and for the second arrangement into eight groups in which l_0 was assumed as 325° and the group solved for $\bar{r}A$ and K in the lower part, the numbers of stars and the mean magnitude being given for each of the 12 groups. The average value of l_0, the direction to the centre, of 326°.4 agrees well with Shapely's value of the centre of the globular cluster system, the centre of the galaxy, of 327°. For the B3, B5 stars brighter than the 6th magnitude both the rotational term and the direction to the centre are indeterminate on account of the

nearness to the sun and the effect of random motions while the rotational term is similarly indeterminate for the brighter stars in the lower group. The average distance of the various groups can evidently be determined if the constant A is known. Independent determinations by Oort of 0.019 and by myself of 0.0155 give a mean value of 0.017 km. per sec. for stars one parsec distance. In other words, stars 60 parsecs or 200 light years distance would give a rotational effect of 1 km. per sec. At this rate the fifth magnitude O to B2 stars are at an average distance of 1,300 and the sixth and seventh at 3,000 light years. The B3 and B5 stars are much nearer being only about 550 and 700 light years respectively for sixth and seventh magnitudes.

A comparison of the observed residual velocities, the radial velocities after the removal of the solar component, with those computed from the values of $\bar{r}A$ and K obtained by the solutions for the four magnitude divisions of the O to B2 stars, given in the lower part of Table I, are shown in Tables II in

TABLE I

ARRANGEMENT AND SOLUTIONS

Two Magnitude Groups—Brighter and Fainter than 6.0

Type	No.	$\bar{m}$	$\bar{r}A$	K	l_0
O to B2	132	4.40	$+4.5\pm0.9$	$+6.2\pm0.7$	$337°.9\pm10°.9$
"	116	6.89	$+15.4$ 1.9	$+0.9$ 1.4	323 .9 6 .9
B3, B5	355	4.85	$+0.7$ 1.1	$+4.6$ 0.7	indet[e]
"	272	6.78	$+3.8$ 0.7	0.0 0.5	$321 .9\pm10 .1$
					$326°.4\pm$ 2 .9

Four Mag. Groups—4.5: 4.5 to 5.5; 5.5 to 6.5; 6.5

Type	No.	$\bar{m}$	$\bar{r}A$	K	ρ'
O to B2	56	3.21	-0.8 ± 1.3	$+6.0\pm1.0$	8.4
"	50	4.96	$+6.7$ 1.9	$+3.8$ 1.2	10.4
"	56	6.00	$+14.4$ 2.3	$+2.1$ 1.6	13.4
"	86	7.11	$+15.2$ 2.5	$+0.1$ 1.8	13.6
B3, B5	91	3.84	$+0.9$ 1.8	$+6.7$ 1.2	8.4
"	197	5.06	-0.7 0.8	$+4.2$ 0.5	7.8
"	157	6.07	$+2.8$ 1.2	$+1.4$ 0.8	8.7
"	176	7.01	$+3.6$ 0.8	-0.5 0.6	10.3

which can also be seen the arrangement into the sub-groups in longitude.

TABLE II

OBSERVED AND COMPUTED FOR O TO B2 STARS

Brighter than 4.5 Mag.				4.5 − 5.5 Mag.			
No.	$\bar{l}$	Obs.	Comp.	No.	$\bar{l}$	Obs.	Comp.
6	91°	+ 3.5	+ 6.8	6	44°	+14.8	+ 6.4
4	132	+ 0.6	+ 6.3	9	73	− 1.4	− 0.1
13	172	+ 4.2	+ 5.3	5	149	+ 2.9	+ 4.7
5	203	+ 9.1	+ 5.2	10	177	+10.3	+ 9.9
6	230	+ 8.8	+ 5.8	9	217	+ 6.2	+ 7.8
5	268	+12.3	+ 6.7	2	286	− 0.6	− 2.7
8	292	+ 9.1	+ 6.7	8	318	+ 1.4	+ 2.2
9	320	+ 3.2	+ 6.1	1	360	− 8.3	+10.1
$\bar{r}A = -0.8$ $K = +6.0$				$\bar{r}A = +6.7$ $K = +3.8$			
5.5 − 6.5 Mag.				6.5 − 7.5 Mag.			
No.	$\bar{l}$	Obs.	Comp.	No.	$\bar{l}$	Obs.	Comp.
7	31°	+17.6	+12.8	4	343°	+27.4	+ 8.9
8	49	+ 9.9	+ 5.2	18	42	+ 7.9	+ 7.0
11	73	−10.6	+ 6.8	13	55	+ 1.6	− 0.1
3	134	+ 1.2	− 3.2	8	89	−20.0	−13.9
7	161	+ 0.7	+ 9.7	12	110	−12.8	−13.1
15	182	+14.6	+16.0	9	147	+ 7.4	+ 1.4
2	272	− 4.8	−11.8	7	167	+ 0.8	+10.7
3	337	+17.0	+ 8.1	15	177	+10.4	+13.8
$\bar{r}A = +14.4$ $K = +2.1$				$\bar{r}A = +15.2$ $K = +0.1$			

It will be noted that the numbers of stars in many of the subdivisions are small and due allowance should be made for probable lack of compensation in the random motions. Nevertheless the agreement for the last three groups where $\bar{r}A$ is determinate is remarkably good, the observed residual velocities follow closely the double wave rotational swing. This correspondence, along with the agreement in the direction to the centre, show conclusively the presence of a differential rotation of the galaxy.

Certain inconsistencies in the solutions and in the values of $\bar{r}A$ indicate that the grouping by apparent magnitude does not necessarily arrange the stars according to distance, that there must be considerable dispersion in absolute magnitude. Another more sensitive criterion for division according to distance has become available from the relative intensity of the "interstellar" H and K lines of ionized calcium. In a recent paper by J. A. Pearce and the writer in Monthly Notices for January, 1930, it is conclusively shown that the diffuse matter in interstellar space, whose presence is indicated and its motion determined from these H and K lines, is uniformly distributed throughout the galaxy at least over the space inhabited by the O and B stars. The more distant the star, the greater the depth of diffuse matter through which the starlight passes, the greater the absorption of the light and the stronger the H and K lines. These "interstellar" lines appear generally only in the hottest, most luminous and hence most distant stars, practically all those of types O to B2 and also some abnormally luminous B3, and B5 stars. There are 226 stars with these "interstellar" lines of measurable intensity and for which the velocities of both the star and the intervening cloud are known, although in 14 stars the lines are so weak and the distance probably so small that they were omitted from the discussion.

The remaining 212 stars were arranged into three groups according to the intensity of H and K as shown in Table III. The columns are self-explanatory and the results show some interesting features which may be briefly mentioned. The rotational term $\bar{r}A$ is very definitely present and well determined in all three groups with probable errors from one sixth to one twelfth its values in both stars and clouds. The average distances of these groups, from the relation of one km. per sec. for stars 60 parsecs, 200 light years away, are 2,000, 3,000 and 5,500 light years respectively while the corresponding distances of the centre of gravity of the intervening diffuse matter are almost exactly half these values, thus clearly showing its uniform distribution. There seems no doubt from the regular increase in value of $\bar{r}A$ with increase in intensity of the

TABLE III

GROUPING ACCORDING TO INTENSITY OF CA+

Group	No.	$\overline{m}$	Range Int.	$\bar{I}$	Stars		Clouds	
					$\bar{r}.I$	K	$\bar{r}.I$	K
1	90	6.35	4.4 to 6.9	6.08	$+10.2\pm1.7$	$+5.2\pm1.2$	$+5.0\pm0.8$	$+0.1\pm0.6$
2	79	6.73	7.0 to 7.9	7.46	$+14.5\pm2.9$	-0.1 ± 2.0	$+6.9\pm1.1$	-0.2 ± 0.7
3	43	6.56	8.0 to 9.5	8.42	$+27.5\pm2.5$	$+3.2\pm1.7$	$+13.7\pm1.2$	-1.2 ± 0.8

interstellar lines and from the larger values of $\bar{r}A$ that the stars are much more critically arranged into distance groups than when apparent magnitude was the criterion. There must hence be a larger dispersion in intrinsic luminosity, in absolute magnitude, than previously supposed. This is evident when for a range in distance of nearly three times, the mean apparent magnitude only changes by 0.2 whereas if the luminosity was constant, the magnitude change would be 2.2.

A comparison of observed and computed velocities shows the great advantage of the more critical arrangement into distance as the agreement is remarkably close, especially when the small numbers of stars and the consequent possible disturbing effects of random motions are considered. It is scarcely possible to imagine that the double wave swing of the observed residual velocities so distinctly shown in all these groups of the most distant stars in the sky, and even more distinctly by the motions of the intervening diffuse matter, can be accidental. If it is not produced by the differential rotation of the galaxy it must be due to some cause which gives a distribution of the residual velocities identical with a galactic rotation. The direction to the gravitational centre as determined from the distribution of velocities of 326°.4 as given previously and 331°.7 in the paper above cited, give in the mean a direction of 329°. This is exactly midway between Shapley's latest value of 327° and Stromberg's "asymmetry" of 61°, corresponding to a direction to the centre of 331°.

It can hence be definitely stated that not only do the residual velocities of the O and B stars follow remarkably closely, and those of the intervening diffuse matter even more closely, the differential swing that would be produced by a rotation of the galaxy but the direction to the gravitational centre, as determined by the distribution of the velocities, coincides with the direction to the geometrical centre of the galaxy. There seems no room for doubt, therefore, that both the O and B type stars and the diffuse matter in the galaxy show distinctly and certainly that the great galactic system is in rotation around a very distant and massive centre, coinciding with the geometrical centre of the system.

TABLE IV

COMPARISON OF VELOCITIES

Intensity of Interstellar Lines 4.4 to 6.9

$\bar{r}A = +10.22$ $\bar{r}A = +5.03$

Group	No.	Mean Long.	Stellar Velocities Observed	Stellar Velocities Computed	Cloud Velocities Observed	Cloud Velocities Computed
1	2	345°	+36.2	+10.8	− 1.2	+ 3.3
2	2	17	+ 5.9	+14.0	+ 4.9	+ 4.9
3	14	38	+10.4	+ 9.9	+ 5.7	+ 2.9
4	11	61	+ 6.4	+ 2.1	− 0.4	− 1.0
5	13	75	− 7.3	− 2.4	− 0.4	− 3.2
6	8	89	− 6.8	− 5.3	− 6.0	− 4.6
7	8	113	− 2.1	− 5.0	− 6.9	− 4.4
8	6	131	+ 4.5	− 0.5	+ 2.7	− 2.2
9	8	156	+ 7.0	+ 8.0	− 1.7	+ 2.0
10	13	174	+14.4	+12.6	+ 4.9	+ 4.1
11	3	205	− 2.6	+ 9.8	+ 3.4	+ 4.2

Intensity of Interstellar Lines 7.0 to 7.9

$\bar{r}A = +14.53$ $\bar{r}A = +6.91$

Group	No.	Mean Long.	Stellar Velocities Observed	Stellar Velocities Computed	Cloud Velocities Observed	Cloud Velocities Computed
1	8	12	− 5.5	+14.3	+ 4.6	+ 6.7
2	12	38	+ 8.9	+ 8.0	+ 6.3	+ 3.7
3	11	51	+ 7.2	+ 1.7	+ 4.8	+ 0.6
4	10	65	− 1.5	− 5.3	− 0.9	− 2.6
5	4	75	− 7.8	− 9.5	− 2.4	− 4.7
6	8	89	−20.2	−13.7	−10.6	− 6.7
7	3	110	−23.9	−13.9	− 9.7	− 6.7
8	3	139	+ 1.7	− 3.2	− 0.3	− 1.7
9	9	160	+10.6	+ 7.2	+ 0.2	+ 3.3
10	10	174	+17.2	+12.2	+ 2.7	+ 5.6
11	1	192	+44.6	+14.4	+13.0	+ 6.7

Intensity of Interstellar Lines 8.0 to 9.5

$\bar{r}A = +27.52$ $\bar{r}A = +13.72$

Group	No.	Mean Long.	Stellar Velocities Observed	Stellar Velocities Computed	Cloud Velocities Observed	Cloud Velocities Computed
1	2	358	+35.7	+24.3	+ 8.2	+ 9.4
2	2	17	+12.2	+29.3	+ 6.1	+11.9
3	6	34	+21.5	+21.3	+ 7.5	+ 7.9
4	2	47	+11.8	+10.4	+ 6.6	+ 2.4
5	12	70	− 3.4	−10.4	− 5.4	− 8.0
6	4	97	−26.8	−23.4	−15.5	−14.3
7	9	107	−28.4	−23.5	−17.7	−14.5
8	4	141	− 4.1	− 0.9	+ 1.2	− 3.2
9	2	169	+13.6	+16.0	+ 1.9	+ 5.3

Certain conclusions of interest may be obtained from the formulæ and results of Oort. The value of the constant A is fairly well determined as 0.017 km. per sec. parsec, but the related constant $B = A - (V/R)$ which is derived from proper motion data is more uncertain. From a value of B of -0.024 km. per sec. per parsec obtained by Oort, the ratio of the gravitational force due to matter collected at the centre to the total force is 0.53. If we assume, as seems reasonable from the motions of the clusters and spirals, a velocity of 300 km. per second for the rotational velocity near the sun, we find R the distance to the centre is 7,300 parsecs and the mass concentrated at the centre about 6×10^{10} the sun's mass. If, however, a value of B of -0.011 as given more recently by Oort is assumed, we get a concentration of mass to the centre of 80 per cent and a distance of 11,000 parsecs. These values are of course largely speculative but the last value of R, the distance to the gravitational centre, is not much smaller than modern ideas of the distance of the sun from the geometrical centre. The period of rotation with R as 11,000 parsecs and V as 300 km. per second is about 230,000,000 years, the corresponding annual angular motion being 0″.006.

The great concentration of mass to the centre of the galaxy and the differential character of the rotational motions, the stars nearer the centre moving more rapidly than those farther out are very suggestive of similarity of the galaxy to the spiral nebulæ. The condensation of the mass towards the centre in the spirals is obvious and it needs only a glance at their characteristic form to show that the assumption of a similar kind of rotation in the spirals to that just shown to be present in the galaxy, immediately and simply explains the spiral arms, which in any position have the exact appearance that would be given if the stars nearer the centre were moving ahead and those further out lagging behind. This is strong additional evidence of the essential similarity in structure of the spiral nebulæ and the greater galactic system and a striking confirmation of the homogeneity of the universe.

THE PHYSICS OF A STAR

By JOHN Q. STEWART

(Read April 26, 1930)

Assuming that a brief fanciful interlude will be pardoned, the company of this audience is requested for a trip to and into the sun. In another century or two, rocket trips to the moon may become no longer fanciful. If engineers can get control of physical processes which will produce energy on the scale of solar and stellar radiation, a number of startling possibilities may become practicabilities by the time Independence Hall has doubled its present age.

To the Sun

However, that may be, trips to the sun are likely to remain purely fanciful. We shall travel at the arbitrary rate of 10,000 miles per hour. Several minutes at this speed carry us up through the last vestiges of the earth's atmosphere; in one day we are crossing the moon's orbit; but it requires 3½ months to reach the orbit of Venus; 4½ months more to reach the orbit of Mercury, and nearly 13 months in all to arrive at the surface of the sun. During the last fortnight of that period we might be travelling through the sun's corona, and at any time during the last 40 hours we might encounter a prominence. We plunge into the chromosphere and ride down through it for an hour. We dash through the reversing layer in a very few seconds, and a few seconds later are lost to sight beneath the photosphere.

Still holding the speed of 10,000 miles an hour, it requires 42 hours longer to reach the center of the sun.

There is one condition with which all passengers must comply—no remarks about the temperature! Our ship is provided with refrigerating machinery, and plenty of power to operate it. To make habitable a space of 10 cubic feet at the

sun's surface, barely enough to hold a man, would require a refrigerating plant rated at about 60,000 kilowatts. At the sun's center, if the temperature there is taken as 40,000,000° K., the power required would be greater by a factor of several thousand trillion. The high cost of living at the center of the sun ought to be notorious. If power cost only 1/1000 cent a kilowatt century—which is all of a billion times cheaper than the usual present rate—the refrigerating bill for keeping a man alive at the sun's center would be about 50 million dollars a minute.

Physics of the Solar Outer Regions

The reversing layer and photosphere form relatively exceedingly thin surface layers, but it is these regions which receive most attention from astronomers. The corona is seen only at total eclipses, and the chromosphere is best seen then. Prominences and flocculi are observable only by special spectroscopic means. Stars are too far away to be seen as disks, and presumably only their reversing layers and photospheres are studied in their integrated light.

Even a brief summary of the physics of the outer regions of the sun would take too much space. The following subjects would require to be included: Physics of the outer and inner corona, of the chromosphere, prominences, and flocculi; temperatures, densities, and pressures; study of sunspots, faculæ, the general and local solar magnetic fields, study of ionization, excitation, rotation, currents, levels, spectral shifts; general and detailed investigation of the Fraunhofer, flash, sunspot, and coronal spectra; identification of chemical elements and compounds in the corona, chromosphere, and reversing layer; quantative chemical analyses; measurements of the radiation of the photosphere; investigations of the general and selective opacity of the solar gases.

General Opacity

Of the various subjects listed, that of opacity is the only one to be discussed in detail here, and even this discussion is

limited. The photosphere possesses general opacity: it is opaque to radiation of all wave-lengths. No thoroughly satisfactory physical theory of this has been proposed, and owing to the difficulties of dealing with vapor at the photospheric temperature of about 6000° absolute, laboratory tests are difficult. Investigators are agreed that the photosphere is gaseous, and at a low density. The density of the reversing layer is less, and that of the chromosphere very much less, while the corona presumably is exceedingly rarefied. To any depth that we can see, the sun is a pretty good vacuum. It owes its solid appearance to the high opacity of the strongly ionized gases of its photosphere.

Observations of the opacity of the ionized vapor formed by "exploded wires" have been made by Anderson.[1] The experiments of Mohler[2] and his collaborators offer a mine of information relating to some of the physical processes which may be involved. So long ago as 1902 J. J. Thomson[3] suggested that the scattering of radiation by free electrons in a gas would result in opacity. After the development of Saha's well-known treatment of ionization it became possible to apply this suggestion quantatively in the case of the photosphere. The application was made by the present author,[4] who showed that the probable transformation of radiation into heat molecular energy as the result of collisions was a more important cause of opacity except at very low vapor-densities. The equations then employed may be somewhat generalized, as follows, starting from relations derived in another paper.[5]

Any theory which treats radiation as a wave phenomenon arrives, under certain restrictions, at the following relation between the opacity coefficient of a gas and its index of refraction;

$$K\lambda/4\pi = \alpha\left\{1 \pm \left[1 - \frac{(\mu - 1)^2}{\alpha^2}\right]^{1/2}\right\}, \qquad (1)$$

[1] J. A. Anderson, *Proc. Nat. Acad. Sci.* **8**, 231, 1922.

[2] F. L. Mohler, C. Boeckner, R. Stair, W. W. Coblentz, *Science*, LXIX, 479, 1929; and other papers.

[3] J. J. Thomson, *Phil. Mag.*, **4**, 253, 1902.

[4] J. Q. Stewart, *Phys. Rev.* **22**, 324, 1923.

[5] J. Q. Stewart, *Journ. Opt. Soc.*, **2**, 581, 1925.

where K is the volume opacity coefficient, μ the refractive index, and

$$\alpha = \frac{3}{16\pi^2} N\lambda^2\sigma \tag{2}$$

Here N is the number of optically active molecules per unit volume and λ is the wave-length. Per unit volume energy is diverted from the primary radiation at the rate KI, of which σKI is supposed merely scattered while $(1 - \sigma)KI$ is transformed into other types of energy. (The possibility of having values of σ less than 1 was not included in the formulæ of the previous paper.)

The principal restriction on (1) is that the number of optically active molecules per cubic wave-length is supposed not great enough to make the "polarization" important. The removal of this restriction would lead to a more general equation. If molecules of several kinds are present, K, μ, and α are to be understood as the respective contributions of any one kind only. The formula 1 is more general than Rayleigh's. It indicates a necessary connection between opacity and refractivity in a vapor.

The chief astrophysical study of general opacity, which has been very useful, has followed another line, however, which was initiated with Eddington's application [1] of Kramer's theory of X-ray absorption. The development of wave-mechanics doubtless will in time result in modifications of this treatment. Wave-mechanics is in many respects closer to classical theory than to the *ad hoc* quantum theories which it has superceded. Eddington's studies of opacity had reference to the deep interior of stars; Milne [2] has followed the same physical line as regards the outer regions. The chief process pictured as significant by these investigators is photoelectric absorption beyond series limits. The calculations, however, refer directly to a microscopic analysis of the corresponding emission, and for their application to opacity reliance is placed on the principle of "detailed balancing" in a thermodynamic equilibrium.

[1] A. S. Eddington, *The Internal Constitution of the Stars*, Chapter IX, 1926.
[2] E. A. Milne, *Monthly Notices*, **85**, 750, 1925.

The classical treatment which leads to equation 1 represents itself as paralleling more closely the actual physics of the situation. Interesting work remains to be done—both theoretical and experimental—before an adequate critique can be constructed. It seems likely that the classical approach to the problem, which assumes the wave-theory of light, and which in general is a macroscopic or field treatment, has been undervalued. Eddington has remarked[1] that refractive index is a "crude macroscopic conception" and "liable to be misleading," and that a macroscopic theory of wave propagation must not be combined with a microscopic theory of absorption. These statements are themselves liable to be misleading. Coefficient of opacity is also a macroscopic concept, and its linkage to refractive index is lost to view in imperfect atomistic treatments.

The contrast between macrocosmic treatment and microcosmic treatment, between wave and photon, field theory and atomistic theory has significance which far transcends the immediate discussion and would interest logicians if philosophers comprehended physics. The honourable problem of *one* and *many* is doubtfully advanced by assigning to *one* the attribute of "crudeness."

Selective Opacity

The reversing layer is transparent except in the wave-lengths of the Fraunhofer lines. It possesses selective opacity only. Following the pioneer work of Fraunhofer, and later Kirchhoff and Bunsen, no progress was made until recent years in explaining the physical processes which produce the Fraunhofer lines. H. A. Lorentz in 1915[2] indicated the process which is now believed primary. His work was neglected by astrophysicists, although similar considerations were advanced by L. Page[3] in discussing the effect of radiation pressure on chromospheric equilibrium. In 1924 Lorentz's treatment

[1] A. S. Eddington, ibid., p. 237.
[2] H. A. Lorentz, *Proc. Amsterdam Acad. Sci.*, **18**, 134, 1915.
[3] L. Page, *Astroph. Journ.*, **52**, 70, 1920.

was combined[1] with considerations developed by Schuster in 1905.[2]

The resultant theory is this: the Fraunhofer lines are produced primarily not by absorption as heat but by scattering. The light which fails to get through the reversing layer in these wave-lengths is diverted by the active atoms in the same manner as the energy of a radio wave is diverted by a tuned antenna. The width of strong Fraunhofer lines is due to the lack of absolute sharpness in the tuning of the atoms. An atom responds slightly to light of frequencies considerably different from exact resonance, and if enough atoms are present per unit column in the line of sight the line is observed as wide. The more atoms, the greater width. The intensity of radiation from the photosphere falls off arithmetically rather than geometrically with distance as light passes out through the reversing layer. Computations based on the observed widths of Fraunhofer lines indicate how many atoms in the sun's reversing layer are concerned in producing them, and so afford a means of making an absolute quantitative chemical analysis of the reversing layer.

Thus the width of the Fraunhofer lines, except for narrow ones where Doppler effect is involved, can be calculated by the same mathematics that treats the forced vibrations of a damped pendulum, or the electrical oscillations of a circuit having inductance, capacity, and resistance.

Observational astrophysical support for these physical ideas appeared in 1925 in a study of winged lines by Miss Moore and H. N. Russell.[3] Further support was given by the important theoretical and observational work of A. Unsöld since 1927.[4] Milne[5] has refined the application.

One of the earliest treatments of the tuning of an atom is that of Lamb, 1899.[6] Julius's theory[7] of the formation of

[1] J. Q. Stewart, *Astrophy. Journ.*, **59**, 30, 1924.
[2] A. Schuster, *Astrophy. Journ.*, **21**, 6, 1905.
[3] Charlotte E. Moore and H. N. Russell, *Astroph. Journ.*, **63**, 1, 1926.
[4] A. Unsold, *Zeits. für Physik*, **44**, 793, 1927; **46**, 765, 1928.
[5] E. A. Milne, *Phil. Trans.* (Bakerian Lecture.) 228A, **421**, 1929.
[6] H. Lamb, *Camb. Phil. Soc. Trans.*, **18**, 348, 1899. (Stokes Commemoration.)
[7] W. H. Julius, *Astroph. Journ.*, **12**, 185, 1900.

Fraunhofer lines is not altogether wrong; he employed the correct physical process, but did not select the correct application. He supposed that the high refractivity associated with atomic resonance to radiation, in combination with a density gradient in the solar atmosphere, resulted in diverting the light. The fact is, however, that the actual relation of the high selective refractivity to the high selective opacity is immediate, as is indicated by the Rayleigh scattering formula, which dates from 1871. The connection of Rayleigh scattering with the Fraunhofer lines was at first rejected by Eddington.[1]

On the laboratory side, there has been the pioneer work of R. W. Wood on resonance scattering, about 1912.[2] Füchtbauer and his collaborators have made a number of relevant and interesting experiments.[3] There have been several other studies, and at Princeton continual experiments have been in progress since Fairley's in 1926.[4] S. A. Korff's work[5] has shown that classical ideas are correct as regards the *form* of the relation of the selective opacity in the D lines to the refractive index of sodium vapor. Whether unmodified classical theory predicts precisely the correct *magnitude* of the opacity is still uncertain. There are indications to the contrary.

The processes involved in the centers of wide absorption lines are still in doubt. There is definite evidence that simple scattering is confined to the edges.[6] Milne at first queried[7] the validity of the application of the classical scattering theory, emphasizing the difficulty already recognized of explaining the illumination at the center of strong Fraunhofer lines if such scattering were the only process effective there.

One very interesting outcome of these studies of selective opacity is the possibility of making a quantitative analysis of the reversing layer. H. N. Russell has brought to bear on

[1] A. S. Eddington, ibid., p. 353.
[2] R. W. Wood, *Physical Optics*.
[3] C. Füchtbauer, *Annalen der Physik*, **71**, 222, 1923, etc.
[4] A. S. Fairley, *Astroph. Journ.*, **67**, 114, 1928.
[5] J. Q. Stewart and S. A. Korff, *Phys. Rev.*, **32**, 676, 1928; S. A. Korff, ibid., **33**, 584, 1929; **34**, 457, 1929.
[6] J. Q. Stewart and S. A. Korff, *Astroph. Journ.*, **71**, 62, 1930.
[7] E. A. Milne, *Monthly Notices*, **85**, 739, 1925.

this problem a method[1] which involves also intensity relations from quantum theory. This method possesses connections with the theory of line width outlined above, but in part these connections are still somewhat obscure.

The Physics of a Star

Studies of the sun's surface layers possess great astrophysical significance, but a discussion which runs literally and metaphorically deeper is requisite before so heavy a title as "The Physics of a Star" can be supported. The leading and pioneer authority in the matter is Eddington, to whom every subsequent student owes profound acknowledgments. Curiously enough, this operator of an "analytical boring machine" —which seems capable of drilling through ions, electrons, and radiation to the center of every star—asserts in effect that a star has no physics of its own. Eddington's physicist on a cloud-bound planet is able to estimate the properties of stars without knowing that stars exist.

A pretty conceit. That it is no more is indicated by the inability of several other investigators to see the actual stars eye to eye with Eddington. If all they had to look at were one another's theoretical stars—!

Fortunately our excellent planet earth is not permanently cloud-bound, and the observing astronomers have collected with immense labor an increasing amount of reliable data about the stars. In perhaps no other study in physical science must so great a range of precise observations be subjected to so intense a degree of theoretical refinement. Sir James Jean's book "The Universe Around Us" affords a splendid summary.[2]

A Few Stellar Data

The *mass* of an individual star can be computed directly from observation only if the star is a component of a binary system. The mass of the sun bears about the same ratio to the mass of a cubic yard of air as the mass of that much air

[1] H. N. Russell, *Astroph. Journ.*, **70**, 63, 1929.
[2] Sir James Jeans, *The Universe Around Us*, 1929.

bears to the mass of a single molecule. Dr. Plaskett may or may not have a mountain named for him, but one-billionth part of the binary which is called Plaskett's star contains enough matter to build mountain ranges the height of Everest all around the equators of a thousand planets like the earth.

The direct measurement with the interferometer of the *angular diameters* of the largest stars is equivalent to the problem of determining with a telescope located in Philadelphia the height of a candle flame in New York. The study of eclipsing variables affords a second means of measuring diameters.

The mean *densities* of stars range from vacuum values up to amazing figures for the white dwarfs. A suitcase full of matter at the density of the companion of Sirius would have a mass of a thousand tons or so. A finger ring of similar density might weigh on earth a hundred pounds.

The measurement of the rate at which *energy* is radiated from the sun and stars gives another class of fundamental data. The solar energy which strikes the earth in two seconds, if it could be utilized, would be about equivalent to the power supplied by the public utility companies of the United States in a year. For the radiation from Sirius received by the whole earth to supply the same energy would require about 700 years.

The surface *temperatures* and *spectral types* of stars comprise other very important classes of observational results. Various by-products follow from knowledge of these.

The problem of the *ages* of the sun and stars is relatively difficult, and the conclusions far from certain. The time scale in stellar astronomy has been vastly increased in recent years, and Jeans speaks of "millions of millions" of years. The rate of development of stars is majestically slow, but the relative numbers and relationships of different types of stars afford hints as to the life history of individuals.

All these data, and more besides, must be employed by the theoretician who would construct a physics of stars.

Researches in Stellar Constitution

The first period of research in stellar physics proper began with the mathematical study of Lane, 1870, and continued with the work of Lockyer, Emden, Hertzsprung, and may be said to have finished with that of Russell about 1913. The well-known *luminosity-type diagram* brings out the "Hertzsprung-Russell distribution" of stars. The relations of brightness, surface temperature, and diameter can also be well shown. Russell gave a well-known explanation of the concentration of stars along certain lines in this diagram; that explanation is no longer accepted, although it has played a very useful rôle.

The second period of research began perhaps with Sampson's indication in 1894 of the importance of the radiative transfer of energy deep in stars, and continued with the work of Schwartzchild, Jeans, Eddington, Fowler, Rosseland, and a number of others. These investigators illuminate the problem from a wide range of physical information. An astrophysically important product of the work has been the *mass-luminosity relation* discovered by Eddington in 1924. The bigger they are the brighter they shine is the stellar rule.

At present the leaders in studies of stellar constitution and "evolution" are Eddington, Jeans, Russell, and Milne. Their theories diverge and at many points disagree. Eddington is in sharp conflict with Jeans and Milne. The situation is altogether too complex for brief description. It may at least not be misrepresented by the following very brief summary of "divergent theories."

Divergent Theories

Russell 1913: Partial explanation lumonosity-type diagram on assumption energy due to gravitational contraction. Fails on modern time-scale. No explanation for the mass-luminosity relation, and in particular breaks down for dwarf stars composed of perfect gas.

Eddington: Detailed and comprehensive study. Discovered, largely explained, mass-luminosity relation. Most stars perfect gas throughout. No explanation luminosity-

type diagram. No special theory of energy source. Particular "model" elaborated. Great reliance on atomic physics.

Jeans: Liquid stars. Explains luminosity-type diagram by stability considerations. No explanation for mass-luminosity relation. Energy supposed due to super-radioactivity, transformation of mass to energy.

Russell 1925: Particular energy hypothesis—"giant stuff" and "dwarf stuff," exhaustible. Fits binaries, clusters.

Milne 1929: New "philosophy." "Observables" only. Questions necessity of Eddington's conclusions about interior. Employs mass-luminosity relation as datum. No explanations.

Desirability of a Macrocosmic Treatment

Notwithstanding Eddington's vigorous criticism of Milne —that "all he knows about the inside of a star is the outside," —it may be that Milne has made a valuable contribution to the basic physics of the situation.

A "new philosophy" might be presented in a somewhat different way. A *field theory* of stars is needed. A star is to the gas of kinetic theory what gas is to its constituent molecules. In kinetic theory the boundary conditions of the gas are fixed, while the molecules are in a sense free. The boundary conditions of a piece of stellar material are not fixed, except to the extent that the state of the star as a whole defines conditions of each part. There must be a minimum of the arbitrary about a star which has been left undisturbed for billions of years.

A star therefore is not quite comparable with a lump of matter examined in a laboratory. Eddington's method of working analytically from laboratory physics is of course legitimate, but requires supplementing by a synthetic treatment. We know very little of the properties of matter under stellar temperatures—still less under stellar adjustments.

A Speculation as to the Source of Stellar Energy

Unquestionably the problem of the source of stellar energy offers one of the most interesting fields for study in theoretical

physics. Energy is becoming more and more important for civilization. The applications of artificial sources of energy will continue to increase. It has been stated—with perhaps uncertain accuracy—that the per capita wealth of the nations parallels their per capita power consumption. The wealthiest people use the most power—and grow wealthier. One hazards the suggestion that economists should examine the feasibility of constructing a "power standard" to replace the "gold standard" in money and banking.

The maximum energies are released deep within the stars. The time rates of release per gram are not large, but the total amounts released are stupendous. Conditions deep in stars are discouragingly different from those obtainable in the laboratory. Nevertheless, while the source of stellar energy remains unknown, to call astrophysics a science wholly devoid of practical possibilities would be inaccurate.

Acknowledgments should be made to Professor Rosseland for a suggestion he advanced in a recent interesting conversation concerning stellar physics. He pointed out the desirability of designing a theory of stellar energy which would permit the Helmholtz gravitational contraction to *control the release* of energy. Because of the long time-scale such contraction cannot be itself the source. The following hypothesis seems to be along such lines.

When a charge of electricity is accelerated, energy is radiated. In the nineteenth century the idea presented itself that there might be a similar effect for accelerated masses. Of course it has never been observed and must be nil or very small. It is interesting to try to renovate the idea and apply it to the interior of stars. There all the matter is continually subjected to intense accelerations, due to the high velocities of molecular thermal agitation and the very frequent collisions. The assumption would be that while a mass is accelerated there is a continuous statistical transformation of its constitutional energy, a continual uniting of protons with electrons, perhaps—that hypothetical process which may be equivalent to the transformation of matter to energy.

If this speculation is correct, to accelerate matter is ultimately to destroy it. Such an assumption regarding the nature of the unknown source of stellar energy has logical advantages over hypotheses which ascribe it to the breaking down in the stars of special types of matter. The latter hypotheses necessarily are special and empirical.

The application of the acceleration idea requires at this stage arbitrary choices. Following the electrical analogue, the energy radiated per unit time from mass m subjected to acceleration a may provisionally be taken as $\alpha m a^2$, where α is a universal constant having the dimensions of time. In the electrical case the corresponding formula is $\beta q^2 a^2$, where q is the charge in electrostatic units and β is $2/3c^3$, c being the speed of light.

One yields to the insuperable temptation of the physicist, 1930 model, and introduces Planck's constant, h, equating the hypothetical α to h/mc^2 where m is some universal mass. Three such offer themselves—that of the electron, the proton, and the nameless $\sqrt{hc/G}$, or 5.45×10^{-5} gram, where G is the gravitational constant. The two former are too small for the present application; but insertion of the latter in h/mc^2 yields $\alpha = 1.34 \times 10^{-43}$ second.

A rough computation of the probable average molecular acceleration of the sun's mass indicates that even this small value of α would account for radiation of solar energy at a rate per gram considerably greater than that observed. The average positive ion in the sun probably has a speed, u, of several hundred kilometers a second, and it is accelerated by collisions at the end of every free path. The free paths average perhaps only 10^{-6} cm. in length. But the accelerations occur in a collision-distance, s, of perhaps a thousand times less than this. Thus the average ion may undergo accelerations of $u^2/2s$, or 10^{24} cm. sec.$^{-2}$, tens of trillions of times per second, each acceleration enduring for less than 10^{-16} second. The average electron moves some 60 times faster, and so undergoes accelerations 3600 times greater and perhaps 60 times oftener.

Even if the formula $\alpha m a^2$ really holds for an isolated mass

in free space, some reduction factor might be expected to enter—as in the electrical analogue—when masses close together are being accelerated in random directions. Then only some mean fluctuation in acceleration might be effective. Such a reduction factor might also account for a paradox which Eddington emphasizes:[1] namely, that the sun radiates 30 times less energy per gram than Capella, although its average density is 620 times greater and its average temperature perhaps four times greater. On the molecular acceleration hypothesis, an increase in temperature or density would favor release of energy, but the "reduction factor" might increase rather rapidly with density. Thus the necessity of assuming exhaustibility of the source would be obviated.

The molecular accelerations which kinetic theory indicates are occurring deep in stars are so extreme—of the order perhaps 10^{21} times the acceleration of gravity for ions, 10^{24} times gravity for free electrons—as to justify the suspicion that these accelerations present a problem rich in physical possibilities.

The hypothetical process outlined here offers to engineers a magnificently inefficient mode of releasing energy. In order to generate only one kilowatt, a mass equal to the earth's would require continuously to be subjected to an acceleration of about 3500 million times that of gravity. Such a conclusion casts something of a shadow on hopes alluded to in our introduction. (But perhaps there are more ways than one of skinning a proton.)

[1] A. S. Eddington, ibid., p. 297.

HEIGHTS IN THE CHROMOSPHERE FROM ECLIPSE SPECTRA

By S. A. MITCHELL

(Read, April 26, 1930)

In the past decade investigations on the structure of the atom have casued much excitement and many thrills in the quiet and well-ordered lives of chemists, physicists and astronomers. It has been said with truth that discoveries have followed each other in such rapid succession that before the printer's ink was dry, the new and at times startling theory was superseded by another theory still newer. The work of the astronomer has supplemented that of the chemist and physicist, particularly along observational lines. The astronomer is able to investigate the sun and distant stars and he finds in these celestial laboratories very high temperatures and very minute pressures far transcending any available in the best equipped physical laboratories. Hence it has come about that in this widening of the observational horizon dealing with the physical, or astrophysical, properties of the chemical atom, the astronomer has become the chief investigator. However, it is undesirable to start any competition or arouse jealousies as to who is entitled to the major part of the credit. Suffice it to say that the combined work of the three sciences, with happiness and harmony reigning everywhere, is a splendid example of what may be accomplished by friendly coöperation.

When an element like iron is heated it first begins to glow and the light then becomes more and more intense as greater and greater amounts of heat are applied. The lines first to appear in the spectrum at the lowest temperatures of iron vapor are easiest to excite and are said to have the lowest excitation potential. As more and more heat is applied, through the electric furnace, the electric arc and spark, other lines ap-

pear, coming into being in groups in various parts of the spectrum. The spectral lines which are produced only at the highest temperatures (and usually highest pressures) are said to have a high excitation potential. The combined work of many investigators has permitted a careful analysis of multiplets, series and electron configurations. With this new information, depending on a knowledge of excitation potentials, the Mount Wilson Observatory has carried out a splendid Revision of Rowland's *Tables of the Solar Spectrum*.

The sun is the only one of the fixed stars for which a reversal of its spectrum may be observed. At the beginning of a total eclipse of the sun and again at the ending of totality, the spectrum of the sun's atmosphere, called the chromosphere, may be photographed. This was first observed by Young at the eclipse of 1870 and was called by him the "flash spectrum." At each succeeding eclipse the flash spectrum is one of the most important objects of investigation. In *Handbuch der Astrophysik*, **4**, 275, 1929, the opinion is expressed that "it is more difficult to secure a perfectly successful photograph of the flash spectrum than it is to obtain an excellent photograph of any other single phenomenon attacked by astrophysical science."

In view of the magnificent work accomplished both on the spectrum of the sun and on the spectra of the elements, it has seemed desirable to carry out a revision of the spectrum of the chromosphere, utilizing for the emission spectrum the same methods employed at Mount Wilson on the absorption spectrum. The spectra of the chromosphere employed for the purpose were mainly those of the writer at the eclipses of 1905 and 1925, the original photographs having been taken with a concave grating used without slit. At the extreme ultra-violet a portion of the spectrum secured by Davidson and Stratton in 1926 was included in the discussion which has appeared in *Astrophysical Journal*, **71**, 1, 1930, where the details are given for a total of 3250 lines of the flash spectrum, covering four thousand angstroms beginning in the violet at $\lambda 3066$.

Of course it is not necessary to state that eclipse photo-

graphs are of no real importance to science unless the sum total of human knowledge is advanced by the securing of them. Total eclipses of the sun are very spectacular phenomena, and there is always a keen interest shown both by the astronomer and by the general public. It is of little value to science for an astronomer to see an eclipse or even to photograph the corona from a fast moving airplane. The latter, if successful, is simply an interesting stunt. Unless the knowledge of the sun can be advanced by eclipse observations and the torch of learning in consequence will burn more brilliantly, then the astronomer had better stay at home and devote his time and energy and money to some more useful purpose.

The flash spectrum supplements the information regarding the ordinary solar spectrum as found in the Revised Rowland especially in two different particulars: (1) in the *intensities* of the spectral lines, and (2) in the *heights*, measured in kilometers, to which each line ascends.

The intensities in the flash spectrum have been estimated on the same average scale as that of Rowland. Many years ago, Lockyer pointed that certain lines are much stronger in the chromospheric spectrum than they are in the sun. As the same lines are much stronger under the hotter conditions of the electric spark than they are in the arc spectrum, Lockyer called them enhanced lines.

Eclipse spectra showed one striking peculiarity of each and every one of these enhanced lines, namely that each line extends to greater heights than are attained by the ordinary, or unenhanced, line of the element considered. It is now nearly ten years since Saha pointed out the reason and gave to the world his important theory of ionization. The method was very simple. At elevations above the surface of the sun there is a slow diminution of temperature and a more rapid decrease in pressure compared with conditions nearer the solar surface. Under the conditions in the higher levels an atom may become ionized by losing one external electron. From the ordinary laws of thermodynamics and a knowledge of heights derived from eclipse spectra, Saha was able to calculate the percent-

age of ionization found for varying conditions of temperatures and pressures. His remarkable theory must be classed as one of the most important of all the long train of researches in modern astrophysics.

Many years before this the astronomers had classified the stars according to their color or spectral type. Saha's theory was now able to explain the appearance and disappearance of lines in the spectra of the stars, and in the explanation to assign a temperature scale to the starry spheres. After stellar distances were ascertained with accuracy it became possible to put all of the stars off at a standard distance and thus derive what might be called the stellar candle-power, but which the astronomer calls the absolute magnitude. When absolute magnitudes and spectral types are plotted one against the other then it is found that the stars divide themselves into two classes called giants and dwarfs. One of the bright stars, Betelgeuse, has a density about one millionth that of water. The companion of the brilliant Sirius, called a "white dwarf" has a density 50,000 times water or two hundred times that of our heaviest solid. As both these stars are gases, obeying the laws of gases, it does not seem to be much of an exaggeration to state that the astronomer has a wide range of variation in the experimental conditions found in his celestial laboratories.

Since the year 1913, when the first extended measurements were made of heights in the chromosphere, there has been a gradual but increasing recognition of the importance of heights in the interpretation of many apparently isolated solar phenomena. Many years ago, through the recommendation of the International Solar Union, a grand coöperative scheme was put into effect of measuring the rotation of the sun by spectroscopic methods. Many observatories, in this country and Europe, engaged in the project. At the time when the plan was put into effect, a spectroscopic line seemed to be a very simple affair, and when high dispersion was used wave-lengths seemed to have a very high degree of precision. It seemed a very easy plan to place the slit of the spectrograph, first on the eastern and then on the western limb of the sun. By changing

the slit to different heliographic latitudes, it seemed to be of the utmost degree of simplicity to derive a very accurate law of solar rotation. But alas, and alack! The results for the solar rotation of every observatory differed from those of every other observatory. There seemed to be not only very large accidental, but also large systematic differences in wave-lengths. The latter were very large in amount. For instance, Adams and Evershed independently found that the period of solar rotation for the red line of hydrogen is 24 days, while for the vapors lying close to the photosphere the rotation period is 25.35 days. This difference must mean that there is a high east wind in the upper solar atmosphere of approximately 400 km. per hour. Other investigators working on spectral lines of different heights and different elements have found similar effects depending on the heights. Hence as a consequence it seems necessary to assume that the specific behavior of the Fraunhofer lines refers to definite restricted levels in the sun's atmosphere.

In investigating the spectra of spots on the sun, Evershed found certain displacements in wave-lengths that have been interpreted as due to the motions of vapors in the solar vortex. St. John of Mount Wilson Observatory has abundantly verified Evershed's work and has interpreted the effects on the basis of heights above the sun. At 1500 kilometers above the solar surface there is a level of inversion. Above this level gases flow into the spot, while below this level the flow is out of the spot, the whole making a vortex gigantic in dimensions compared to anything found on the earth.

With the advance of knowledge concerning atomic structure, particularly from the investigation of series relationships, multiplet groups and excitation potentials, we have been enabled to understand more completely the complexity of the problem. A line in a spectrum is no longer the simple affair it was once thought to be. If information of value is to be derived, wave-lengths must be known with the very highest degree of precision. The element iron, so common on the earth, has been very thoroughly investigated. Comparisons of

wave-lengths in the laboratory and on the sun have yielded most interesting results. Iron is very abundant in the sun. There are many lines in the iron spectrum, both strong and weak, throughout a great range of wave-lengths. Recent work by Russell and others has given much information regarding multiplet groups and excitation potentials, the latter covering a wide range in values. Very accurate wave-lengths in the laboratory, by the use of the vacuum arc and interferometer methods have been carried out by Burns at the Allegheny Observatory. When differences in wave-length are taken in the sense *sun minus vacuum arc* (the greatest of care being taken to ensure that all wave-lengths are on the same absolute system) it is found that there are systematic differences which persist throughout the whole extent of the spectrum, the wave-lengths in the sun being always greater than they are in the vacuum arc. When these persistent shifts to the red are correlated with excitation potentials and heights from eclipse spectra, interesting conclusions are drawn. The strongest lines of iron, being those of low excitation potential, reach the greatest heights. For these lines the red shift is the greatest. For weaker lines of high excitation potential the red shift is much less, corresponding to smaller heights. The primary cause for the systematic differences between high-level and low-level iron lines is the heights to which the lines ascend.

St. John has attempted to explain the red shift in the sun compared to the vacuum arc in the laboratory as due to the principle of relativity enunciated by Einstein whereby wave-lengths are greater in the sun on account of the higher value of gravity attraction. There appears to be abundant evidence that St. John has indeed found relativity effects in the sun, for the average displacement is that demanded by the Einstein theory. When the systematic differences, sun minus vacuum arc, are corrected for the relativity shift, there still persists a plus correction in wave-length of 0.003 angstrom for the high-level iron lines at 2000 km. above the solar surface, but a negative correction of 0.003 angstrom is found for the

low-level lines at 250 km. elevation. Investigations in atomic structure tell that many atoms are involved in the intense high-level lines while many fewer atoms take part in the low-lying lines of feebler intensity.

Hence it is necessary to assume that there is a circulation in the sun's atmosphere, the high-level atoms settling down to the sun with a velocity at the 2000 kilometer level of 0.2 km. per second, while at the low 250 km. level the atoms are rising from the sun at approximately the same speed. The number of atoms of iron involved in the rising and falling are about equal to each other, the atoms rising being spread through many feeble lines while those descending from above are concentrated in far fewer lines each of low excitation potential.

A MILLENNIUM OF BIBLICAL HISTORY IN THE LIGHT OF RECENT EXCAVATIONS

By WILLIAM F. ALBRIGHT

(*Read April 25, 1930*)

DURING the past ten years the study of Palestinian archæology has made very great progress, and now rests upon an entirely different basis of method and interpretation from what was the case in 1914. Three factors are mainly responsible for this happy state of affairs. In the first place must be mentioned the vital fact that Palestine is now a mandate of Great Britain, whose government has taken an enlightened interest in furthering the cause of archæology. This it has done in several ways: first by establishing a department of antiquities, with a British director and a trained staff, secondly by establishing an international archæological advisory board, consisting of the representatives of the most important national groups and archæological institutions in Palestine, thirdly by making the formalities connected with the granting of a permit much less cumbersome and tedious than they used to be in Turkish days. While the task of securing a permit to excavate is thus far simpler than it formerly was, the scientific requirements have become more stringent, so that it is easier for reputable organizations and harder for irresponsible ones to secure permits to dig.

The second important factor in this progress is the great increase of archæological interest in America, which has reached its climax with the splendid gifts of Mr. John D. Rockefeller Jr., through Professor Breasted, for the Oriental Institute of the University of Chicago, and for the Palestine Museum, now under construction in Jerusalem. The $2,000,000 donation to the latter, half of which will be applied to the construction and equipment of the museum (including the library), while the other half will be reserved for endowment,

ensures a suitable home for the antiquities found in the excavations and falling to the share of the Palestine government. Two major archæological enterprises have also been launched by American institutions: the excavation of Megiddo by the Oriental Institute of the University of Chicago, now under the direction of Mr. P. L. O. Guy, an able young English archæologist, and that of Beth-shan, where the University of Pennsylvania Museum has carried on eight campaigns, the last five of which have been directed by a gifted young Australian, Mr. Alan Rowe. Unhappily the Beth-shan excavation has now been interrupted for an indefinite period, owing to the difficulty of continuing to work, season after season, in the climate of the Jordan Valley. Besides these elaborately organized expeditions there have been several smaller American undertakings, of which we may mention those which have been most concerned with the pre-Christian period: the excavation of the acropolis of Gibeah of Saul (Tell el-Fûl) by the American School in Jerusalem, under the writer's direction; two campaigns (with a third in the summer of 1930) at Tell Beit Mirsim (Kirjath-sepher) under the patronage of Dr. M. G. Kyle and under the writer's direction; three campaigns at Tell en-Nasbeh (probably Ataroth), directed by Dr. W. F. Badè of the Pacific School of Religion; two campaigns at Beth-shemesh, directed by Dr. Elihu Grant, with the assistance of Dr. C. S. Fisher. American money has also been given freely for the partial support of other excavations in Palestine since the war, especially at Shechem (Balâtah), where a German expedition has been at work.

The third vital factor in producing the satisfactory situation which now exists is the new spirit of unity and continuity which prevails among the workers in this field as a whole. Before the World War there was very little collaboration among scholars of different nationalities and confessions, all interested in Palestinian archæology. Nor were there—with one exception—any who contributed to the stability of this science and the security of its achievements by specializing in it, and thus ensuring the continuity and coherence of research. The one

exception was Père H. Vincent, of the Dominican École Biblique in Jerusalem, who, though then a young man, was already a scholar of distinction. Even R. A. S. Macalister, the most active archæologist in the field, was primarily interested in Celtic studies. The English and German excavators followed different routes, employed different methods, paid little or no attention to each other's work, and did not attempt a synthesis. To be sure, the lack of definiteness in the chronology of excavations was largely due to the fact that scientific method was in its infancy, but if properly cultivated it would undoubtedly have developed much more rapidly. Macalister, easily the most gifted of the field archæologists, worked alone at Gezer, during five campaigns on this site, with the natural result that his views became more one-sided, while the few errors with which he started became cumulative with the passage of time. The chronology of Jericho set up by Sellin and Watzinger was nearly a thousand years wrong at a crucial point, the date of the "red city," the last Canaanite settlement on the site, which should have been placed between the seventeenth and the fifteenth centuries B.C., instead of between the ninth and the seventh, as Watzinger has himself recently acknowledged.[1] At Gezer Macalister's dating became more and more inconsistent; in his final publication he contradicted himself repeatedly with regard to the date of the same object, at the same time that he systematically reduced Iron Age dates by two or three centuries. Small wonder that when historians tried to systematize the results of pre-war excavation they found such a hopeless confusion that they were often led to express the view that archæological discoveries in Palestine were nearly valueless unless accompanied by written records!

Thanks to the rapid development of institutions and societies in Jerusalem, several of which are mainly devoted to archæological research, there are now media by which one scholar may readily become familiar with the methods and discoveries of others. Through the international Palestine

[1] See *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, vol. 80, pp. 131–6.

Oriental Society, now in its tenth year of life, and through the interlocking activities of the more important institutions devoted to archæological studies, especially the French École Biblique, the British School of Archæology, the American School of Oriental Research, the Archæological Department of the Hebrew University, the German Protestant and Catholic Institutes for Palestinian Studies, the Pontifical Biblical School, a constant interchange of views accompanies the publication of discoveries. The Department of Antiquities of the Palestine Government, with its rapidly growing museum, soon to be housed in the Rockefeller building, as noted above, provides a clearing house for archæological information, the value of which will steadily increase as time goes on.

The history of scientific excavation in Palestine dates back only to 1890, when Flinders Petrie applied the methods of studying a mound which had been developed by Schliemann and Dörpfeld at Troy, and the methods of employing pottery for chronological purposes which he himself had worked out in Egypt, to the site of Tell el-Hesi in the south of Palestine. Though his ideas were decried by the leading archæologists and historians of the day, they have gradually prevailed. Today they are regarded with suspicion only by those who know nothing of ancient Palestine. While, as observed above, there has been much loss of time because of the desultory and fragmentary character of Palestinian archæology in the past, we are now rapidly reaching the point where the system of sequence dating can be employed by historians of ancient Palestine with absolute security. Back to about 1700 B.C. any group of characteristic vases or potsherds, all contemporaneous, can be dated with a margin of error of a century—two centuries in extreme cases. If the pottery is sufficiently abundant, it is already possible, in most cases, to date it with an even lower margin of error. It does not, of course, follow that all such datings are correct; at present there are only a few scholars, such as Vincent (*facile princeps*), Fisher (who has an elaborate corpus of Palestinian pottery in

preparation), and Guy (the excavator of Megiddo), whose judgment may be relied upon. During the past nine years the writer has seen many verifications of the dates assigned by archæologists of this small group to pottery, when tested by later discoveries, or by the judgment of others, arrived at quite independently. It is true that we can only give very rough dates for the pottery of the ages preceding 1700 B.C. In fact, the exact nature of neolithic (chalcolithic) pottery was not known until 1929, and the dates assigned by eminent authorities to pottery of the Early Bronze have proved to be fundamentally erroneous. This situation is due, of course, to the fact that there has been virtually no excavation in sites and strata older than the eighteenth century B.C.

Of all periods in the long history of Palestine, where sedentary human occupation stretches over some 6,000 years, none has been so illuminated by recent archæological research as that comprised between the latter part of the Middle Bronze Age and the end of the second phase of the Early Iron, 1600–600 B.C. Spurred on by the wide-spread interest in the historical background of the Old Testament, archæologists have devoted themselves with particular zeal to sites and levels which might be expected to yield most information bearing on it. Since 1921 the writer has directed four campaigns of excavation at Tell el-Fûl (Gibeah of Saul) and Tell Beit Mirsim (probably Kirjath-sepher), besides making soundings on other sites, and acting as archæological adviser to various expeditions, especially to the Danish excavators at Shiloh. He has also visited all the excavations conducted since the war, and has studied scores of other sites belonging to the Old Testament period. He does not, therefore, feel the need of making any apology for undertaking the task of synthesis, though one must, of course, recognize that there are still many lacunæ in our knowledge, and that many cultural phases and ethnic movements are very imperfectly understood.

If we transport ourselves back to the seventeenth century B.C., we find ourselves in the age known to biblical students

as that of the Patriarchs, while historians designate it as the Hyksos period, and archæologists refer to it as the latter part of the Middle Bronze Age. The Hyksos period was inaugurated by a great barbarian irruption, or perhaps by a series of such irruptions, the exact course of which is still unknown. It is now certain that the Indo-Iranians played a dominant rôle in this movement, which presumably originated in the basins of the Caspian and the Aral, but they were hardly the only peoples to participate in it.[1] At all events, they were responsible for the introduction of chariot-warfare into the Near East. With the horse-drawn chariot came a new military aristocracy, of feudal character, which transformed the social and political organization of western Asia and Egypt.[2] The pathway of the barbarian invaders is marked by a line of great rectangular fortresses, often half a mile or more in length, a line stretching from the plains of Transcaspia across northern Persia and Mesopotamia, Syria and Palestine, into Egypt.[3] The fortresses were really fortified camps, surrounded by massive ramparts of *terre pisée* ("beaten earth," varying in consistency from hard clay to a mass almost as hard as concrete). They were employed primarily to shelter the wagons and chariots of the barbarian army, and proved so well adapted for this purpose that some of them continued in use for centuries. The best example of a Hyksos fortified camp in Palestine is the site of Hazor, the most important Canaanite city of Galilee, which is just a kilometre long, and four hundred metres wide.[4]

It is now becoming increasingly clear that the traditions of the Patriarchal Age, preserved in the book of Genesis, reflect with remarkable accuracy the actual conditions of the

[1] Cf. Eduard Meyer, *Geschichte des Altertums*, vol. II, 1 (2nd ed.), pp. 33 ff.; Albright, *Journal of the Society of Oriental Research*, vol. 10, pp. 243–54.

[2] Cf. Meyer, *op. cit.*, pp. 44 ff.; Albrecht Alt, *Die Landnahme der Israeliten in Palästina*, p. 8; also a forthcoming paper by the writer on Mitannian *maryannu*, "Chariot-warrior," in *Archiv fur Orientforschung*.

[3] See *Journal of the Society of Oriental Research*, vol. X, pp. 245–54. The material has now been greatly increased, and the writer's conclusions have been accepted by many archæologists, among them Garstang, Petrie, Du Mesnil du Buisson, etc

[4] See Garstang, *Annals of Archæology and Anthropology*, vol. 14, pp. 45 ff.; Albright, *Zeitschrift fur Alttestamentliche Wissenschaft*, 1929, p. 12.

Middle Bronze Age, and especially of the period between 1800 and 1500 B.C. The ancestors of Israel are represented as semi-nomadic Hebrews, migrating into Palestine from northern Mesopotamia, where their home had been, and where their base continued to be.[1] In Palestine they wandered in the central highlands, between the Canaanite and Amorite towns which were scattered sparsely over it. Various non-Semitic peoples, like the Horites and the Hittites, appear as the inhabitants of enclaves, isolated one from the other.[2] The early traditions of Israel preserve important references to an invasion of Palestine by a coalition of Mesopotamian princes, among whom the northern barbarians, or Gôyîm (Babylonian Manda), appear,[3] as well as to a Hebrew migration into Egypt, in connection with the rule of a Hebrew vizier.[4] One isolated passage (Num. 13 : 22) says that Hebron was built seven years before Tanis, the Hyksos center in the Delta. Several passages refer in unmistakable terms to persistent traditions of a partial early conquest of the central highlands by the Hebrews, but most of these memories have been completely dimmed by the splendor of the official story of the conquest under Joshua.[5] Recent archæological discoveries show that hitherto obscure references to social customs of the Patriarchal Age must be explained in the light of contemporary Horite practises in northern Mesopotamia.[6] The researches of the American School in Palestine have proved conclusively that the picture of the state of settlement in the hill country of Palestine, as found in Genesis, is in strict accord with the

[1] See *Journal of Biblical Literature*, vol. 43, pp. 385–93.

[2] Cf. *Journal of the Palestine Oriental Society*, vol. II, pp. 125 ff. The Hebrews came in during this period, and not during the preceding, Amorite period, before the eighteenth century. For this age, on which most important light has now fallen from Egyptian sources, see *Journal of the Palestine Oriental Society*, vol. VIII, pp. 223–56.

[3] See *Journal of the Society of Oriental Research*, vol. X, pp. 236 ff., 255 ff.

[4] New light on the Joseph story has recently been shed, as will be shown in a different place.

[5] Cf. *Zeitschrift für Alttestamentliche Wissenschaft*, 1929, pp. 11–13; *Bulletin of the American Schools of Oriental Research*, No. 35, pp. 3–6.

[6] For general orientation see Chiera and Speiser, *Annual of the American Schools of Oriental Research*, vol. VI, pp. 83 ff., and for the most remarkable biblical parallel so far discovered in these documents see Gadd, *Revue d'Assyriologie*, vol. 23, p. 127.

archæological facts, and is not a product of fanciful archaizing on the part of priestly scribes.[1]

Unhappily, we know little about the political history of the Hyksos Empire, but Palestinian archæology elucidates the fragmentary documentary material in a most satisfactory way. The "rulers of foreign lands," as the Hyksos princes called themselves, settled the ethnic groups which had formed a part of their movement, or which had been swept up into its course just as the Huns swept up the Goths and the Vandals, here and there in different parts of their empire.[2] The biblical accounts of the conquest picture vividly how complex the ethnic structure of Palestine was, with at least half a dozen different non-Semitic peoples occupying different parts of it. The cuneiform tablets from Palestine found at Taanach, at Shechem, and especially at Tell el-Amarna in Middle Egypt, accentuate the biblical statements, showing that most of the names of the local princes were non-Semitic, the greater part being Indo-Iranian (Sanskrit), while Horite (Hurri) and Cappadocian ("Hittite") names are by no means lacking. It cannot be accidental that the Indo-Iranian names are most abundant in the plains of Haurân and elsewhere in northern Palestine, where conditions are best for horses and chariots.

The Canaanite civilization arising from the fusion of all these elements was very complex in character. It was essentially feudal, being based on the domination of an aristocracy of chariot-warriors, under one of their number as prince. Each local prince called himself by the simple designation *awîlu* (noble) when addressing his overlord, but from his subjects, and even from his equals, he demanded the appellation "king." The best term would be "baron," a word which has the same original significance of "(free)man" which is possessed by *awîlu*. Under this baron and his knights (*maryan*, Canaanite *na'ar*), whose only difference from equestrian knights was that they fought from chariots instead of from the backs of horses, were the serfs (*awîlût ḫupši*), who

[1] Cf. especially *Annual*, vol. VI, pp. 67 ff.
[2] Cf. Alt, *Landnahme*, *loc. cit.*

tilled the soil and built the castles of their lords. Each castle stood on a natural hill, or on an artificial mound, composed of the débris of previous occupations, and was surrounded by massive fortifications. At no time in the history of Palestine do we find such massive walls as in the feudal age, when there was a constant struggle between the robber barons for domination, and serfs were always available for the corvée. The fortress walls were frequently over fifteen feet thick, and were protected at their base by massive sloping revetments, from twenty to thirty feet in height.

Thanks to recent discoveries at Beth-shan and Tell Beit Mirsim, our knowledge of Canaanite religion has been revolutionized. This religion was fundamentally identical with the Syro-Mesopotamian religion of the third millennium, though influenced both by Egypt and by Asia Minor. It is not yet possible to say whether it was affected perceptibly by the Indo-Iranian cult of the divine order (*arta*), or of the gods Indra and Yama, lords of the storm and of the underworld, whose names appear in theophorous compounds mentioned in the Amarna tablets. The two great deities of the Canaanites were the god of heaven, who generally combined the attributes of a solar divinity and of a storm-god, and the great mother goddess, who appears variously as a celestial being, generally the moon, and as a chthonic divinity. The god of heaven received, of course, different names in different places, but the general term *Ba'al*, "lord," prevailed over all more specific ones. As god of the storm he was *Hadad*, while another name, belonging more intimately to his function as patron and donor of fertility, was *Dagan*. A chthonic form of the same god, with special reference to his connection with the underworld, both as a destroyer and as giver of fertility, was *Rashap*, who was worshipped at Beth-shan as lord of Hades, in lion shape.[1] The mother-goddess received the names *'Ashtart* and *Ashirat*, both found all over the Semitic world. These names belonged to her primarily as queen of heaven, but as a chthonic deity she was called *'Anat*, the goddess of fate, who apportioned life

[1] For the nature of this god see Albright, *Haupt Anniversary Volume*, pp. 144 ff.; Rowe, *Museum Journal*, vol. 20, pp. 48 ff.

and death from her abode in the lower world.[1] 'Anat was worshipped either as a serpent, or as a goddess with a serpent coiled about her legs, or climbing up one leg. The serpent was the symbol of life and death, and also represented the fecundizing element, without which the earth would remain sterile. Because of her close resemblance in many respects to 'Ashtart, 'Anat was early identified with her, and became queen of heaven as well as earth goddess. Finally, the two deities were combined by the Aramæans into the composite figure of Atargatis, the great Syrian goddess.[2]

These deities, with the minor divinities of their circles, were worshipped by the Palestinians of the Bronze Age either in temples, following the custom of the older sedentary population, or in open-air high places, according to the practise of the younger, semi-nomadic element. The cult was extremely debased, that is, it preserved very crude, primitive elements, in an age of comparatively advanced civilization; the combination of crudity and sophistication inevitably leads to corruption. The sexual aspect of the cult of fertility was abnormally developed, with the concomitant appearance of temple courtesans, worshipping a goddess whose most common appellation was "the courtesan" (*Qadesh*). The temple courtesans were both male and female; the priests were, at least generally, eunuchs (*kemarîm*), like the later Galli of Syria and Asia Minor.

After the expulsion of the Hyksos from Egypt, the Egyptians entered upon their heritage, and for just four centuries maintained the tradition of their Asiatic empire (c. 1550–1150 B.C.). Meanwhile, Hebrew tribesmen were gradually gaining control of the central highland between Jerusalem and the Plain of Esdraelon, as well as of certain other less important sections of the country. After their

[1] On 'Anat see Albright, *American Journal of Semitic Languages*, vol. 41, 73 ff., and Vincent, *Revue Biblique*, vol. 37, pp. 540 ff. Vincent has pointed out that she must have been a serpent-goddess; for the serpent-goddess of Tell Beit Mirsim see *Bulletin*, No. 31, pp. 3, 6; the serpent on the thigh of the goddess appears at Bethshemesh (Grant, *Beth Shemesh*, p. 35) and on a large terra cotta statuette from the Wâdi Mughârah, found in 1929 by Miss Garrod, but not yet published.

[2] See the paper in the *American Journal of Semitic Languages*, cited in the preceding note.

initial successes, between the break-down of the Hyksos empire and the time of Tuthmosis III (early fifteenth century),[1] they do not, however, appear to have gained much until the latter part of the thirteenth century, when the Israelite confederation invaded Canaan, sweeping over it like a storm and dividing the hill country among its tribes. *Israel* is first mentioned in the Egyptian inscriptions about 1230 B.C., when it takes the place of *Hebrews* as the designation of the semi-nomadic tribesmen who occupied the hill country.[2] After the Israelite conquest, the Hebrews, both older occupants and newcomers, began to settle in towns, utilizing the recently developed art of building cisterns to enable them to build towns and villages all over the hill country, which had been sparsely peopled until then. Very few of the Israelite towns in the hill country were occupied by an older Canaanite population; most of them, like Gibeah of Saul, show no traces of occupation before the end of the Bronze Age, when the Israelite conquest took place.[3]

It cannot be emphasized too strongly that the Israelite conquest brought with it a radical transformation in the character of the culture. The Hebrews were vividly conscious of the gulf which separated them from the older Canaanites; in theory, though perhaps seldom in practise, they considered the Canaanite population as *ḥêrem*, that is, as devoted to extermination. Imperfectly as this principle was carried out, it still proved of the greatest importance, making the Israelite culture independent in vital matters from the very beginning. The Hebrews kept their old tribal organization intact down to the time of David, and in certain respects it survived as late as the Babylonian Exile. This tribal organization, based on a patriarchal theory, but greatly modified in practise by customs surviving from a more primitive matriarchal state of society, was essentially democratic; every able-bodied Israelite was equal to every other one, and the tribal nobility enjoyed very little real power. The change in organization appears con-

[1] See *Zeitschrift fur Alttestamentliche Wissenschaft*, 1929, pp. 11 f., and the references there given.

[2] *Ibid.*, pp. 12 f.

[3] See *Annual*, vol. IV, pp. 44 f.

cretely in the curious fact that the average thickness of the walls of early Israelite towns was seldom more than a third that of the older Canaanite fortresses, on whose ruins the Israelite settlements were founded. Under a democratic order, it is naturally impossible to erect as massive fortifications as would be possible under the system of the corvée, where serfs were impressed into service. The very word for serf, peasant bound to the soil, received the new meaning "free peasant," whence "freeman in general" (*ḥopshî*).[1]

The Israelite conquest also brought an abrupt change in religious beliefs and practises. While it is quite true that the simple religion of the semi-nomadic Hebrews was unable to resist the influence of the complex and attractive religion of Canaan, but adopted many beliefs and practises from it, it must be remembered that the Israelite confederation was a direct result of the preaching of a new faith, to which it remained more or less deeply attached. Since the exact character of the religion of Yahweh, as preached by Moses, is a controversial subject, upon which archæology is not yet able to speak decisively, we need not go into it here.[2] Suffice it to say that it must have been essentially monotheistic, and have already possessed a sufficient number of ethical, social, and ritual prescriptions to have given Judaism a good start in the direction of its later development. In any case, archæological evidence is absolutely opposed to many of the views of the dominant school of historical criticism with regard to the origin and evolution of Mosaism. The recent excavations at Shiloh have proved conclusively that it was an important Israelite town, abandoned before 1000 B.C., after a destruction by fire, and not reoccupied until after the Exile. While it is hardly probable that the site of the tabernacle, which was believed in later times to have been a large tent, will be discovered, it is already certain that the account of its history, as given in our sources, is essentially correct. An inscribed

[1] See *Journal of the Palestine Oriental Society*, vol. VI, pp. 106 ff.

[2] For the writer's view cf. provisionally *Journal of Biblical Literature*, vol. 43, pp. 370 ff. The situation is now being cleared up by the studies of Alt and others. That Moses was a thorough monotheist seems to me practically certain, though hardly susceptible of direct proof at present.

weight, of about the eleventh century B.C., shows that writing was employed at Shiloh in the days of the Judges.

The excavations at Tell Beit Mirsim and elsewhere have shown that the cult of the mother goddess was not entirely given up, but, on the other hand, they have shown that the Astarte plaques of the Bronze Age were replaced by little busts set on pedestals, exactly like the toys of the period in general character, and but little more sacred. All of the high places described in archæological publications dealing with Palestinian excavations except one (at Gezer) have proved to be ordinary private houses, stables, or other edifices without sacral purpose. On the other hand, the altars of incense found at Gezer, at Shechem, Megiddo, and Tell Beit Mirsim (eleventh or tenth century B.C.) are now known to be the *ḥammanîm* against which the later priestly writers of Judah inveigh, because of their association with heathen cult.[1] However, they probably represent a phase of the cult of Yahweh in Israel. Except at Megiddo and at Shechem no temples of the Early Iron Age have yet been excavated, and these towns were both Canaanite rather than Israelite, as is well known.

Some two or three generations after the Israelite conquest, while the Israelites were still engaged in conflict with the remaining Canaanite towns, there was a new irruption of foreign tribes, this time from the north and west—the migration of the Sea Peoples. For centuries the Egyptian Pharaohs had employed mercenaries from the northern shores of the Mediterranean, the forefathers of the later Sardinians, Tyrrhenians, Lycians, and others. Some of these Mediterranean mercenaries were settled at garrison towns, like Beth-shan, where their burials have been found, showing a barbaric adaptation of Egyptian practises. Finally, however, at the beginning of the reign of Ramesses III (cir. 1180–1150 B.C.),[2] which followed a generation of more or less complete anarchy in Egypt, a number of the "peoples of the sea," as the

[1] See *Zeitschrift fur Alttestamentliche Wissenschaft*, 1929, p. 13, and *Journal of the Palestine Oriental Society*, vol. IX, pp. 50 ff.

[2] For his date cf. *Zeitschrift für Alttestamentliche Wissenschaft*, 1929, p. 9, n. 3

Egyptians called them, invaded the coasts of Egypt and Palestine. At the same time, roughly speaking, the Hittite empire was overthrown by a barbarian irruption from the northwest and northeast, which forced various Anatolian peoples into Syria. The relative sequence of movements eludes us, so we do not know certainly whether the Egyptian inscriptions are right in suggesting a joint movement, both by sea and by land. At all events, many of the Canaanite towns of the coastal plain of Palestine were occupied by the maritime invaders, led by the Philistines, who took the most desirable section of the country, between Joppa and Gaza. Excavations have so far been carried on in only one of the Philistine towns, Ashkelon, and while the soundings here were not extensive enough to lay bare any area of the pre-Hellenic city, a stratum of ashes was found to separate the Canaanite town from the superimposed Philistine stratum, a fact which points to a destruction of the former by the invaders. Ramesses III claims to have won a great victory over the invaders, but he confesses that he settled them as his subjects, *i.e.*, confirmed their title to the conquered territory, subject only to the payment of tribute to Egypt. After his death Egypt once more fell into a state of anarchy, and for over two centuries remained too weak to attempt to assert her domination over Palestine.

In the Israelite towns of the Shephelah, the low hill country between Judah proper and the Philistine plain, we find Philistine influence dominant from about the middle of the twelfth century B.C. At Tell Beit Mirsim we found that the Israelite town of the second stratum from the top exhibited three periods, a pre-Philistine one (cir. 1220–1150), a Philistine one (cir. 1150–1000) and a post-Philistine one (cir. 1000–920 B.C.), though the third one is not so clear as the other two. At Beth-shemesh, Mackenzie made similar observations with regard to the first two of these periods, so there can be no doubt that the Philistine conquest fell considerably later than the Israelite one.[1] The Philistine influence upon Israel was

[1] *Ibid.*, pp. 8 ff.

undoubtedly very important, both directly and indirectly. The Philistines were by no means barbarians; they unquestionably came from some region of the Ægean, and were to some extent under Cretan influence. They introduced iron into general use in Palestine, though they held a monopoly of iron manufacture until the time of Saul. Iron had, indeed, been long known, but until the eleventh century B.C. it was too scarce and expensive to be used for ploughs or sickles, to say nothing of other tools. Most of the iron in use came from Pontus, where it was a jealously guarded monopoly of the Hittites in the thirteenth century. Early in the Iron Age deposits of it were discovered in the Lebanon, after which it became abundant. In the excavations at Gibeah an iron plough tip was found in the fortress of the time of Saul (cir. 1020–1000 B.C.). The Philistines also introduced an entirely new type of pottery, of Ægean origin, as well as new styles in clothing. The use of the fibula, or safety pin, for fastening clothes was certainly brought in by the Philistines. The Philistine armor, as worn by Goliath, is characteristically Ægean.

The Philistines perpetuated Ægean institutions in their confederacy of five towns, each ruled by a tyrant, and their strongly commercial orientation. It was not long before they were in control of the trade routes running north toward Syria and east into Transjordan. The related peoples who occupied most of the old Canaanite towns of the coastal plain and Esdraelon were doubtless not altogether averse to the protection against Israelite incursions which the Philistine hegemony brought them. The Israelites, on the other hand, felt the foreign yoke severely, and finally revolted successfully under the leadership of Saul, the first king of Israel.

Saul was still a peasant, and it was reserved for his successor, David, to establish monarchical institutions. Our excavations in the fortress of Saul's time on the acropolis of Gibeah showed a considerable amount of rustic comfort, but no luxury whatever. The art of construction was still extremely rude, and a gulf separates the masonry and architecture of

Saul's period from that of Solomon's. In fact, the age of Saul was characterized by a deliberate opposition to Philistine culture; not a single example of Philistine pottery was found in the stratum of this period at Gibeah.

Up to 1928 it was possible to say that the age of David and Solomon was archæologically quite obscure. In that year P. L. O. Guy, director of the excavations at Megiddo, carried on under the auspices of the Oriental Institute of the University of Chicago, discovered the stables built by Solomon in order to accomodate his chariot horses. The construction of the stables is remarkably strong, and the cement paving which was liberally employed in them gives a surprisingly modern appearance to their ruins. Mr. Guy has been able to show that similar structures found before the war at Tell el-Ḥesi and Taanach, but never understood, are in reality precisely similar, and date from the same period. At Hazor Garstang has since found other stables, apparently also of the Solomonic age. The great expansion of Israelite military power under David and Solomon may be appreciated when we learn that stabling space for three hundred horses has been found at Megiddo alone. The masonry of the stables is practically identical with that of the buildings of the time of Ahab (cir. 870–850 B.C.), excavated by Reisner at Samaria, constructed a little more than half a century after Solomon's death. There can be no reasonable doubt that this type of construction was borrowed from Phœnicia, though positive evidence is still lacking. The spread of Phœnician influence in Palestine at this time is attested both by our documentary and by our archæological materials. The diffusion of Phœnician influence was greatly aided by the fact that the language of northern Israel was practically identical with Phœnician, though differing in important points from the dialect of the south, which we know as Biblical Hebrew. We have very important documents of North Israelite speech in the ostraca found by Reisner in the ruins of a building of Ahab's time at Samaria, as well as in the Mesha Stone, set up by a king of Moab who had successfully revolted from Israel. Dussaud is probably

right in pointing out that the tradition of Solomon was carried on, not by his descendants in Judah, but by the Israelites of the Northern Kingdom, who revolted from his son. The administrative organization introduced by Solomon was, at all events, maintained in the Northern Kingdom, as we know from the ostraca, as well as from other indications.[1]

Israel, however, was no longer in the direct line of Jewish evolution; that privilege became more and more exclusively the property of Judah. The excavations of recent years, especially in the top stratum of Tell Beit Mirsim, have enabled us to understand the nature of Judæan culture much better than before. Tell Beit Mirsim, we must remember, was a typical peasant community in the Iron Age, and no signs of wealth may be expected there. In the seventh century B.C. it had a population of about five thousand souls, but during a good part of the year the inhabitants lived in booths outside the walls, just as the modern Arabs of the district spend only two or three months in their town of Dûra, and live during the rest of the year in tents, caves, or booths on or near their land. The principal industry of the place was the preparation of woolen cloth for export; the town was situated on the edge of a great sheep-raising district. Innumerable loom weights show that practically every house possessed a hand loom. Five dye factories were excavated in the first two campaigns, four of which were almost exactly alike in their disposition. In the middle of a large room were two massive stone basins, in which the cloth was dyed. By their side were generally shallow basins of cement, while a line of fallen stones of considerable size, each one hollowed out, pointed to an original pipe-line for convenient bringing of water from a neighboring cistern. In the corners were always two large hole-mouthed jars, partly full of lime, which is still used today in the dye industry. The characteristic feature of these dye-plants is the amount of labor which was devoted to the fabrication of the massive stone basins and pipe-lines, which could just as well be made of earthenware, as was indeed true of the basins found in one dye-plant.

[1] See *Journal of the Palestine Oriental Society*, vol. V, pp. 25 ff.

When we turn to the construction of the houses, we find the same situation. Each house, as a rule, contained, on the ground floor, a large room with four massive stone pillars, usually monolithic in character, and averaging seven feet in height. When we recall that the inhabitants of the houses were simple peasants, and consider the construction of modern Arab peasant houses, we are struck with the native physical energy and enterprise of the Israelites, who would go to so much trouble to build their houses. We also find that sanitary conditions were better in the Israelite town, despite its crowded condition, than in an Arab town or village of the same general type. The central room of the ground floor was provided with smaller rooms opening from it, which served as magazines, or the like. A stone staircase, outside the house, gave access to the second story, which was generally built of brick and wood, and where the occupants slept. The floor of the ground story was generally, though not always, paved with stone or plaster, and the walls were plastered with good lime plaster. The ceiling was of wood, as were also the doors. Streets were narrow, but were generally paved with cobblestones, though the latter were not laid in patterns. There were numerous cisterns, but all were provided with channels of cement for the purpose of conveying the water from the roofs of the houses, as well as with settling basins of cement, in order that the water might be partially purified before entering the cistern. In short, sanitary conditions were much superior to what they are in modern Arab communities, though there was no satisfactory arrangement for the disposal of sewage. The sanitary tabus of early Hebrew legislation also contributed greatly to the advantage of public health. Christianity and Islam have both given up the sanitary tabus of Judaism, a development which has led to deplorable results in poor and depressed communities.

Both the documentary sources and the archæological evidence enable us to follow the gradual evolution of Israelite society from a simple tribal democracy, entirely devoted to farming and herding, to a typical monarchical state, where

manufacture and commerce played increasingly important rôles. The development of commerce began actively under Solomon, who was himself a great merchant prince, through whose influence the Israelites undertook elaborate commercial expeditions into Egypt, Syria, Arabia, and even to the Indian Ocean and to the Phœnician outposts in Spain. It was, however, in the Jewish state of the eighth century B.C. that commerce became most highly stressed. The mere fact that the town of Tell Beit Mirsim, located in the south of Judah, was devoted to the manufacture of woolen goods, shows to what an extent economic specialization had developed. We know from the pre-exilic records preserved in the book of Chronicles that certain towns were occupied by members of the guild of metal-workers, others by potters, still others by manufacturers of linen goods, etc. This high degree of specialization shows that conditions were economically complex, and that commerce must have attained a high degree of development. To what extent the pre-exilic Jews participated in Phœnician commerce is a moot question, but they were in any case very strongly influenced by it.

One of the most interesting illustrations of the originality displayed by the pre-exilic Judæans is the fact that in the eighth century B.C. they invented a system of currency unknown, apparently, in all the neighboring countries. Payments of taxes in kind were a feature of ancient taxation; in Israel crop taxes were paid in grain, wine, and oil, for convenience. The wine and oil, which formed the staple taxes, were, from the eighth century down to the Exile, placed in standardized jars, with the capacity of a bath (*i.e.*, about eight gallons), each jar having been stamped on one handle, before it was baked, with the seal of the royal administrative district. These jars then circulated as standard values.

It is thus quite certain that the commercial expansion of Jewry under the conditions of the Diaspora was by no means so sudden as used to be thought, but was simply an acceleration of an otherwise normal evolution. Our new comprehension of this evolution is naturally of great value for the

just evaluation of the work of the prophets. It cannot be accidental that the great prophetic activity of the eighth century B.C. was coeval with a burst of commercial and industrial acitvity. This new life brought with it a great extension of the bounds of Hebrew thought, both cosmically and politically, but it also involved a transformation of the old, relatively democratic, social organization of the early monarchy into a more complex industrial system, where the upper classes oppressed the lower classes. Even where there was no real oppression, the introduction of new luxuries, enjoyed only by the rich, and the formation of caste barriers gave the poor a feeling that they were being oppressed. The prophets tried to bring about a social reformation; some of them even tried to bring back the primitive semi-nomadic life of early Israel, much as Mahatma Gandhi is endeavoring to do in India today. It was this reaction to simplicity of life and law which was probably responsible for the collection in writing of the Hebrew traditions which we find in the J and E documents, from the eighth and early seventh centuries, as well as in Deuteronomy, which represents an effort to restore the primitive Mosaic legislation at the end of the commercialized seventh century, where it appears oddly anachronistic, despite certain modern touches, especially in cult legislation.[1]

[1] In order to avoid misunderstanding, the writer regards a brief statement of his views as advisable. He believes that the Jews, both pre-exilic and post-exilic, traced the origin of the law (*tôrah*) back to Moses, and that the codes of the Pentateuch were just as honestly intended to reproduce Mosaic legislation as the Moslem *ḥadîth* was supposed to reflect actual teachings of Mohammed. In practise, however, it goes without saying that an orally transmitted law would steadily grow, both by absorbing later legislation, and by explanatory additions. The actual Mosaic nucleus of the codes is, therefore, presumably small, though any serious attempt to reconstruct it is at present quite impossible. In all likelihood there was a more or less authoritative body of oral tradition, both historical and legal, which was transmitted by the priesthood of Shiloh. Certain documents incorporated in P may well go back to written sources of the eleventh century or older. After the downfall of the Shiloh cult, consequent on the destruction of the place by the Philistines, the priests were scattered, and the body of authoritative tradition, above referred to, passed through a new and complex period of oral transmission, partly in the south, partly in the north. About the end of the eighth century these two recensions or "documents," known respectively as J and E, were put into writing, followed in the seventh century by the compilation of JE. Deuteronomy, dating in its present form from the end of the seventh century, has a much older nucleus, consisting probably of the narratives and laws which were handed down at the old shrine of Gilgal, near Shechem, in so far as they supplemented the material of Shilonic origin

In the foregoing pages we have tried to give an idea of the light shed by archæology on the broad sweep of Hebrew history, from the Patriarchal Age to the Exile, without going into details. There are still many lacunæ; our knowledge is just beginning to be sufficiently exact to make synthesis possible. We have avoided really disputed questions, so far as possible, though it must be confessed that the field of biblical history is still so enshrouded by religious and anti-religious prejudice that it is impossible to make any statement without meeting opposition from some quarter. However, this very situation makes the vigorous pursuit of archæological research in Palestine a matter of the greatest possible interest and importance to all philosophically minded persons.

(JE). The Priestly Code (P) never seems to have had a strictly independent existence, but represents new material added by the priests of the Temple at Jerusalem to JE, whether before or after the fall of Jerusalem is not yet certain. But even P does not attempt to describe the usages of the Temple, but rather to go back to the days of Moses. Some of the matter included in P is older and more valuable than that of JE, despite the relative lateness of the work as a whole. Happily for us, in spite ot the effort made by the compilers of these documents to give only authentic material, dating to the time of Moses, they have not succeeded, but have given many religious laws and prescriptions of obviously late date, by comparison of which the relative age of the documents may be established. It is only by full recognition of the antiquity and tenacity of oral tradition in Israel, on the one hand, and of the late date at which our material was compiled and edited, on the other, that we can do justice to the history and religion of more than a thousand years' span which are imbedded in that marvelous work known as the Pentateuch.

STRATIGRAPHY AND PALEONTOLOGY OF THE PALEOCENE OF NORTHEASTERN PARK COUNTY, WYOMING

By G. L. JEPSEN

(*Read April 24, 1930*)

I. ABSTRACT

THREE new Paleocene faunal horizons were discovered in the "Fort Union" of the northern part of the Bighorn Basin of Wyoming by the Princeton Scott Fund Expeditions of 1927, 1928, and 1929. The lowest is above the Lance dinosaur-bearing beds in the base of a heavy buff sandstone (heretofore regarded as Lance by the U. S. Geological Survey), and has afforded among other Puerco genera, *Oxyacodon*, *Eoconodon*, *Conacodon*, and *Loxolophus*. Two hundred feet higher a second level yields Torrejon forms, such as *Chriacus*, *Tetraclænodon*, and *Tricentes*. Above this a third new horizon which was locally quarried contains among other genera *Carpolestes*, *Leptacodon*, and *Ectypodus*, which correlate it in part with the Montana Bear Creek and the Colorado Tiffany. The fourth and uppermost horizon of the Paleocene in this locality is the "Clark Fork," which has been described by earlier workers.

From an erosional disconformity at the top of the Lance upward through the Paleocene to the locally angular unconformity between the "Clark Fork" and the Gray Bull, no pronounced structural or stratigraphic break has been observed. The sediments are a series of somber clays and sandstones.

This single section contains representatives of all the Paleocene mammalian faunas previously known in North America, and is confined by the Lance below and the Gray Bull above. These two facts afford important new evidence

of the relationships of the elements within and bounding the "Fort Union."

II. INTRODUCTION

The Bighorn Basin

The great oval Bighorn Basin of Northwestern Wyoming is a sediment-filled structural concavity by virtue of the slow growth and development of a mighty surrounding horseshoe rim of high mountains, narrowly open to the north. Its greater axis directs northwest.

Geographic convenience divides the mountains that fashion this arcuate welt of the Middle Rocky front into several ranges. The Absarokas are its western side, their southern end bending eastward [1] and merging with the Owl Creek Mountains, which, in turn, continue as the Bridger Range. These latter two divisions of the mountain rim trend roughly west and east of north, respectively, and shape the back arch of the horseshoe, opposite the open northern end. The eastern side is formed of the lofty Bighorn Mountains that sweep north from the Bridger Range and extend into southern Montana as the Pryor Mountains, which decline into the plains.

The northward-flowing Bighorn River admits itself to the basin through a southern notch (Wind River Canyon) between the Owl Creek Mountains and the Bridger Range. It courses across the southern end of the basin, diagonal to the structural axis, and continues northward near the foothills of the Bighorns, collecting, among its tributary veins, the Gray Bull and the Shoshone [2] rivers. Having drained all of the structural basin except the very northern part, it cuts through the eastern rim of mountains, where its canyon is the physiographic basis for dividing the Bighorns from the southern end of the Pryors, and meanders across the Montana plains to discharge into the Yellowstone.

Clark's Fork of the Yellowstone River drains the northern

[1] Formerly this curve was designated as the "Shoshone Range."

[2] "Stinking Water River" in early narratives and reports, in reference to some of its feeder springs, west of Cody, whose water is charged with hydrogen sulphide gas.

segment of the structural basin. It rises in the Absarokas, and is distinguished by perpetual spring and snow-fed western branches, like Line and Little Rocky Creeks, and intermittent feeders such as Little and Big Sand Coulees from the desert badlands on its eastern side. It meanders out between the horseshoe ends onto the Montana plains, parallels the Bighorn and, also, flows into the Yellowstone.

The Clark Fork collecting area is separated from that of the Bighorn River system by a low oblique ridge that extends from Heart Mountain irregularly northwestward to the western side of the Pryor Mountains, possibly as a reflection of a buried structural cross-warp.[1]

A meticulous accuracy in describing the region would dictate a reference of the northern hollow, or Clark Fork Basin, to the Yellowstone Basin, and a retention only of that area drained by the Bighorn River under the name 'Bighorn Basin.' However, to conform to the practice in most of the regional literature and to the local geographic usage, 'Bighorn Basin' is regarded for the present purpose as a generalized structural term.

Other great broad furrows that may be likewise lobed lie north of the Bighorn trough, and also east, between the Bighorns and the Black Hills.

Most of the basin's surface is elevated more than 4000 feet above sea level, and the average is nearly 5000 feet. The Bighorn River enters at about 4200 feet and drops 400 feet in its 120 miles through the basin to 3800 feet at its northern canyon where it leaves the basin.

Some peaks on the eastern side of the arcuate mountain welt rise above sea level 13,000 feet,—9000 feet above the

[1] Osborn, H. F., and Wortman, J. L., "Fossil Mammals of the Wasatch and Wind River Beds," Collection of 1891, *Bull. Am. Mus. Nat. Hist.*, Vol. IV, Art. XI, pp. 137, 144, 1892. Eldridge, G. H., "A Geological Reconnaissance in Northwest Wyoming," *U. S. G. S. Bull. 119*, pp. 15, 46, 1894. Fisher, C. A., "Geology and Water Resources of the Bighorn Basin, Wyoming," U. S. Geol. Survey Prof. Paper 53, pp. 4, 36, 1906. Matthew, W. D., and Granger, W., "A Revision of the Lower Eocene Wasatch and Wind River Faunas," *Bull. Am. Mus. Nat. Hist.*, Vol. XXXIV, Art. 1, p. 3, 1915. Thom, W. T., Jr., and Dobbin, C. E., "Stratigraphy of Cretaceous-Eocene Transition Beds in Eastern Montana and the Dakotas," *Bull. G. S. A.*, Vol. 35, map, p. 482, showing contours on Dakota Sandstone, 1924.

lowlands of the basin. The north and west sections are lower, but ascend to 4000 and 6000 feet, respectively, above the adjacent stream valleys.

Several conspicuous eminences in the basin mount to 1000 or 1500 feet above the average level. Heart Mountain reaches 8080 feet; Squaw Buttes, 6200 feet; Tatman Mountain, 5800 feet; McCulloch Peak, 6200 feet.

In general, due to the gradual definition and elevation of the enclosing mountain arc, and despite the modifying erosive and deformative accessories, the older sediments lie exposed along the periphery of the basin, and as the geologic center is approached, the observer traverses successively younger beds, with certain omissions, from the Cambrian into and through part of the Lower Eocene, all having uptilted, truncated outer edges. Only the younger formations in the very middle of the basin have extensive surface exposure; the others appear as narrow concentric bands which frequently extend for scores of miles along their strike, although they may be only a few hundred feet wide.

Many hogbacks or cuestas, with gentle inner dip-slopes and steep mountainward eroded escarpments, have developed along the basin's margins, as an erosional compliance to the general dip of the alternating resistant and soft strata toward the structural center. The regularity of this centrical dip is occasionally modified or interrupted, however, by local folding. Some of the resulting perched anticlines and domes are now eroded into hollows, such as the crater-like Polecat Basin in the southwest corner of T57N, R98W (Map, Fig. 2).

Physiographic types that dominate the middle of the basin are incised desert badlands, interrupted aggradational and degradational plains, and gravelled upland and river terraces. Playas develop in the ill-drained sections.

A sensitive adjustment to rainfall and elevation divides the flora of the basin into sharp ecologic zones. The border ridges that flank the mountains are irregularly forested with conifers and dicotyledons, but on the badland and plain sections the vegetation consists of small species meagerly

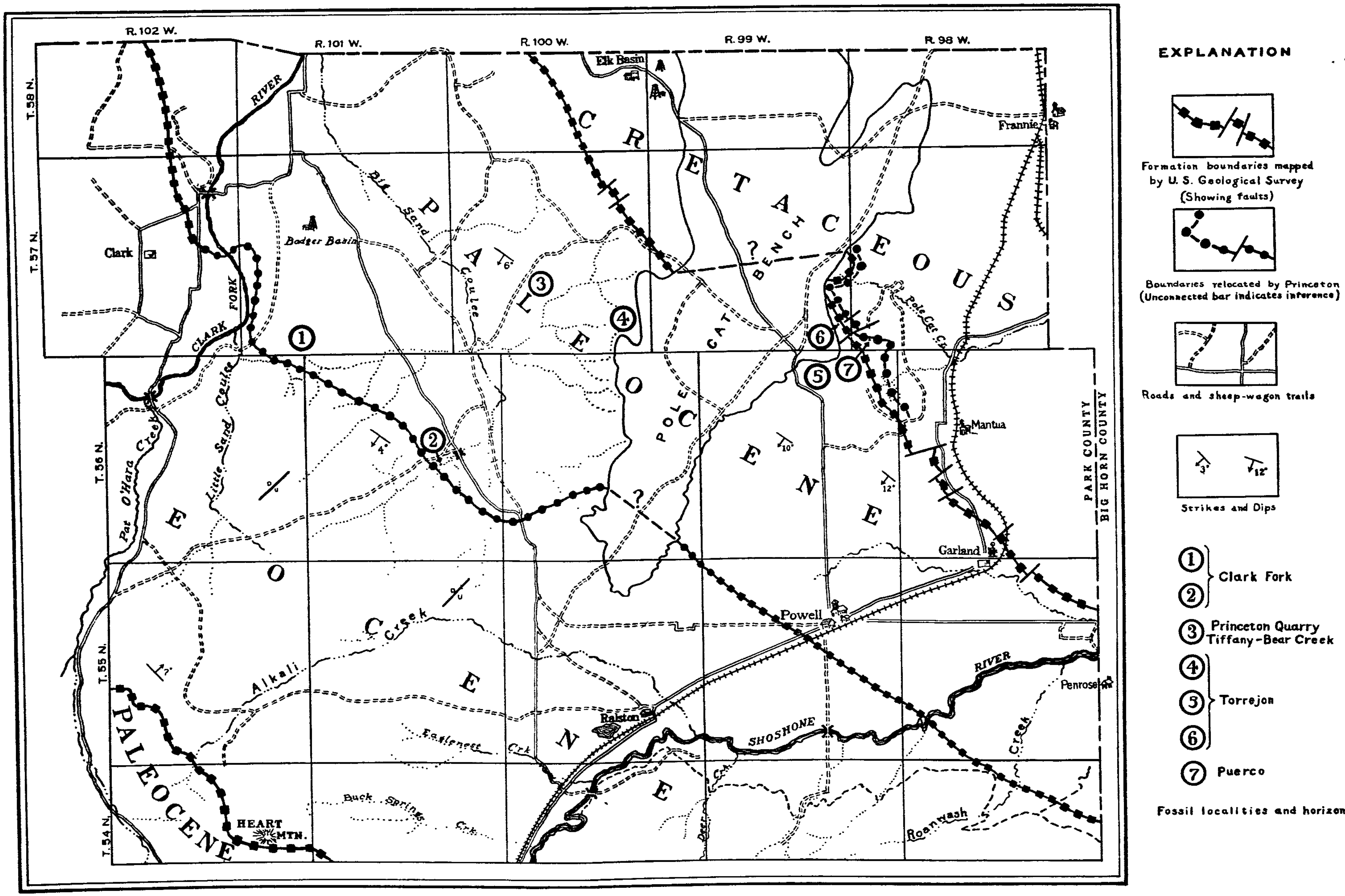


Fig. 2.—Enlargement of shaded area of Fig. 1, with the geologic age of the strata indicated. Circles mark important fossil localities.

distributed. Several varieties of cactus and sagebrush, little hardy yuccas and grasses are characteristic forms, and cottonwoods and willows stud the sloughs and stream banks.

Weather conditions are widely variant, usually severe, and whimsically unsteady,—as in most intermontane basins. The average annual precipitation is from 7 to 10 inches.

Area of Investigation

The stratigraphic and paleontologic results of three field seasons of study in a limited area of "Fort Union" (Paleocene) and adjoining formations in Northeastern Park County, Wyoming, are presented in this paper. Text Fig. 2, as an enlarged projection of the shaded portion of the Wyoming

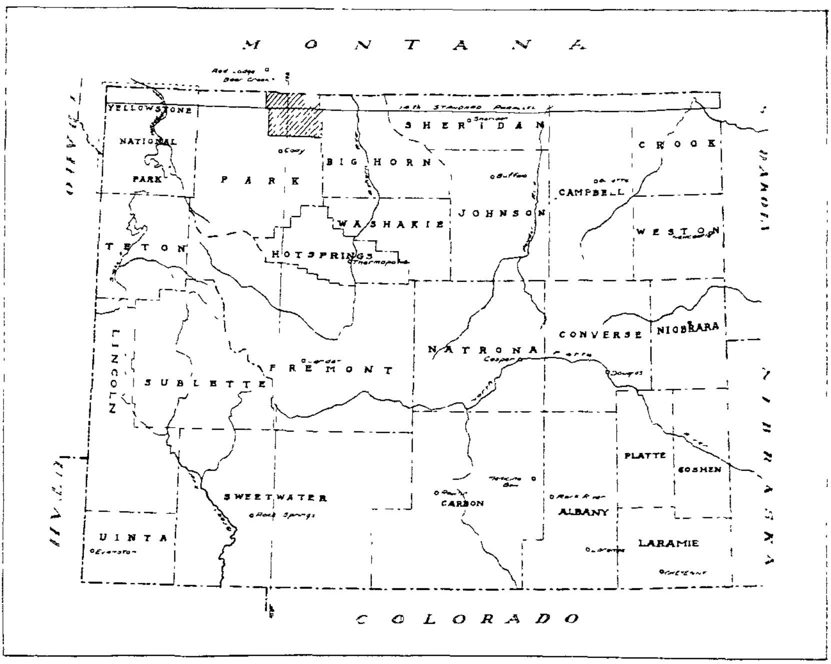


Fig. 1.—State Map of Wyoming, with the area of this investigation shaded.

state map (Fig. 1), illustrates the locality's geography and general geology, the latter modified from the U. S. Geological Survey's map of Wyoming, edition of 1925. The divide

between the Clark Fork Basin and the Shoshone River Basin extends, roughly, from the southwest to the northeast corners of the map. In the northeastern and central parts of the region, the beds are dipping gently to the southwest, observable in the relationships of the strips labeled "Cretaceous," "Paleocene," and "Eocene." But the "Paleocene" deposits appear again in the extreme southwest corner, indicating that the dip is there reversed, with the beds declining toward the axial plane of the structural trough whose deepest part underlies the Eocene and trends northwest-southeast.

The Lance formation is included in the topmost Cretaceous, in accordance with the consensus of opinion among vertebrate paleontologists, but contrary to the practice of the U. S. Geological Survey of placing it as questionably Tertiary.

Formation names are not used upon the map, but the divisions are indicated by depositional ages, "Paleocene" and "Eocene" epochs and "Cretaceous" period, to avoid splitting the latter into the several thin formations which outcrop in the northeast part of the region, below the Lance.

In the area covered by the map the Paleocene and Eocene sediments seem conformable, and if the two series are separated by a hiatus, it is not revealed by an angular discordance. A few miles south, however, at the southwest base of McCullough Peaks, and also along the eastern margin of the basin west of the town named Basin, the edges of the Fort Union strata (Paleocene) are truncated by overlying Eocene beds which dip less steeply toward the structural center of the basin.

ACKNOWLEDGMENTS

Many generous friends have aided in the preparation of this exposition, and to them the author now expresses deep and sincere gratitude.

The devoted supervision of Professor Sinclair, and the counsel of Dr. Thom in the field and in the laboratory, have greatly decreased the labor. Indeed, nearly every member of the Princeton Department of Geology has lent information which the author lacked.

Diligence in the field rewarded Joseph F. Page '30 and James W. Cooke '30, members of the Princeton Scott Fund expeditions of 1928 and 1929, by the discovery of some of the finest fossil specimens herein described.

Dr. G. G. Simpson and Mr. Walter Granger and Mr. Barnum Brown have courteously helped to identify the fossils and compare them with types in the American Museum of Natural History. This same favor was extended at the U. S. National Museum by Mr. J. W. Gidley and Mr. C. W. Gilmore.

Maps and charts of the U. S. Reclamation office at Powell, Wyoming, and of the U. S. Geological Survey, have afforded assistance in expediting field work and in supplying data which are incorporated in the map, Fig. 2, in this report.

Residents of the Bighorn Basin extended hospitality and zestful coöperation. Mr. Albert Wardell of Otto helped liberally, sometimes even at the expense of his own occupation; and Dr. J. C. F. Siegfriedt most affably demonstrated many points of interest in the vicinity of Bear Creek.

The illustrations of the fossils present their own testimony to the accurate skill of the artists, Mrs. Sheldon Howe, who drew most of them, and Mr. Loris Russell, who executed the figures of *Microcosmodon conus* and *Titanoides gidleyi*.

III. ANTECEDENT TERTIARY STRATIGRAPHIC WORK IN THE BASIN

The magnificent mammalian faunas from the basin, and the stratigraphic data which permit horizon differentiation of its Tertiary sediments, have been assembled since 1881, when Dr. J. L. Wortman discovered the fossiliferous character of the lower Eocene and secured an excellent collection for Professor Cope. Much of this material was described in Cope's titanic Tertiary Vertebrata,[1] wherein the mammal-bearing beds of the basin were all considered as "Wasatch," a name applied by Hayden in 1869 to a group of formations near Wasatch station, Utah, on the U. P. R. R.[2]

[1] Cope, E. D., Report of the U. S. Geological Survey of the Territories, Vol. III, Tertiary Vertebrata, Book I, 1884.

[2] Hayden, F. V., (Third Annual) Preliminary Report of U. S. Geological Survey of Colorado and New Mexico for 1869, p. 90.

Three years later, in 1884, a party from Princeton, led by Professor Scott, obtained a small fossil assemblage during a most strenuous field season,[1] and ascertained the presence of *Lambdotherium* in some of the upper levels near Meteetsee.[2] Wortman resumed his Bighorn Basin exploring and prospecting in 1891, when he conducted the first expedition for the newly-founded Department of Mammalian Paleontology of the American Museum of Natural History. At that time the difficult wagon journey from Red Lodge to the mouth of the Gray Bull River, a distance considerably less than one hundred miles, required nearly a week! Wortman's stratigraphic report that followed, 1892, still included all of the mammal-bearing beds of the basin in a single formation and faunal horizon, the Wasatch, which rested upon "older secondary rocks." He supported the soon-discredited hypothesis that the sediments of the basin had accumulated upon the bottom of a lake which drained prior to the beginning of deposition of Wind River beds in the "upper basin" to the south.[3] He obtained additional valuable fossil material in 1896. Loomis collected very profitably for Amherst in 1904, and subsequently published an illuminating analysis of the lithologic and faunal facies, demonstrating that the sediments were mostly fluviatile and not lacustrine in deposition.[4] This same conclusion was reached by Fisher[5] in his studies in 1904 and 1905 of the geology of the Bighorn Basin, when he calculated the "Wasatch formation" to be 2000–3000 feet thick, and stated that it rested unconformably upon 5000–7000 feet of "Laramie and associated formations." Loomis concluded that the "uppermost 1000 feet of the Wasatch beds belong to the same period as the Wind River in the basin south of Owl Creek Mountains," because of the absence of the tapiroid

[1] Scott, W. B., Unpublished Memoirs, Vol. 3.

[2] Unpublished data communicated by Professor W. B. Scott.

[3] Wortman, J. L., "Geological and Geographical Sketch of the Wahsatch Exposures in the Bighorn Mountains," *Bull. Am. Mus. Nat. Hist.*, Vol. IV, Art. XI, pp. 139–144, 1892.

[4] Loomis, F. B., "Origin of the Wasatch Deposits," *Am. Jour. Sci.*, 4th Ser., Vol. 23, pp. 356–364, 1907.

[5] Fisher, C. A., "Geology and Water Resources of the Bighorn Basin, Wyoming," U. S. Geol. Survey Prof. Paper 53, 1906.

perissodactyl, *Homogalax* (*Systemodon*), and the presence of *Lambdotherium*, a small primitive cursorial titanothere. In southwestern Wyoming and adjacent Utah, Veatch observed that above the Laramie, and separated from it by "a pronounced period of folding and erosion" there were three formations, the Evanston, the Almy, and the Fowkes, which possessed "stratigraphic isolation" and were separated from the Knight formation by a lesser "period of folding and erosion."[1] The Wasatch, as defined by Hayden, included Veatch's Almy, Fowkes, and Knight. The latter alone yielded mammalian remains and was called the "Coryphodon Wasatch" by Veatch.

Systematic work in the Wind River and Bighorn Basins by Walter Granger and W. J. Sinclair in 1910 and 1911 afforded a large collection for the American Museum and resulted in important stratigraphic determinations. There is no necessity to review here all of their tentative correlations and suggestions; but from the top of Fisher's "Laramie and associated formations" they collected a distinct, though limited, fauna with a Fort Union (Paleocene) aspect[2]. In the Wind River basin they divided the typical "Wind River formation" into an upper member, the Lost Cabin, and a lower, Lysite, member, both of which they subsequently identified in the Bighorn Basin, correlating Loomis' Wind River with the Lost Cabin member. Granger worked in the basin again in 1912, and summarized all the information about its Tertiary formations in 1914.[3] The general term "Wasatch" had now been so buffeted about and alternately stuffed and deleted that it no longer conveyed a precise meaning, and Granger refrained from using it for any of the basin's members. The uppermost horizon which he discussed

[1] Veatch, A. C., "Geography and Geology of a Portion of S. W. Wyoming," U. S. Geol. Survey Prof. Paper 56, 1907.

[2] Sinclair, W. J., and Granger, Walter, "Eocene and Oligocene of the Wind River and Bighorn Basins," *Bull. Am. Mus. Nat. Hist.*, Vol. 30, pp. 83–117, 1911. "Notes on the Tertiary Deposits of the Bighorn Basin," *Bull. Am. Mus. Nat. Hist.*, Vol. 31, pp. 56–67.

[3] Granger, Walter, "On the Names of Lower Eocene Faunal Horizons of Wyoming and New Mexico," *Bull. Am. Mus. Nat. Hist.*, Vol. 33, Art. XV, pp. 201–207, 1914.

is the Lost Cabin, containing *Lambdotherium*. Below this, but above the *Homogalax* (*Systemodon*) bearing beds, is the Lysite, which he found, upon a faunal basis, to be the equivalent of Veatch's "Knight." Below, conformably, are the Gray Bull beds which yield *Homogalax* abundantly. Between the base of the red-banded "Gray Bull," as connoted by Granger, and the top of the gray Paleocene strata, he observed about 200 feet of red-banded beds wherein he found no *Homagalax*, and hence considered a new subdivision advisable. For these beds he proposed the name "Sand Coulee," and called the previously mentioned underlying Paleocene beds "Clark Fork," giving as their thickness 500 feet, and observing that the unconformity at their top is only local, and that "they rest upon beds whose age is not positively determined, perhaps Cretaceous, perhaps Fort Union." He was unable to report "whether or not there is an unconformity at this point."

A geologic map of Wyoming, compiled by members of the U. S. Geological Survey and published in 1925, indicated that in the basin the Lance formation occurred just beneath the Fort Union and outcropped as a narrow bordering band.

In 1926 mammalian fossils were discovered in a coal mine in the Fort Union at Bear Creek, Montana, between the Pryors and the Beartooth Mountains. The specimens, which Dr. G. G. Simpson subsequently described [1] revealed forms closely allied to some of the species from the Upper Paleocene Tiffany, of Colorado, and suggested that the horizon might be just prior to the "Clark Fork" phase, or a part of it.

In addition to the work accomplished by professional geographers and geologists, hosts of amateurs have contemplated the basin for its commercial potentialities and made some valuable calculations, but the results of their activities are usually unavailable or disorganized.

Previous to 1929, several writers had suggested indefinite correlations of the Bighorn Basin lower "Fort Union" with the

[1] Simpson, G. G., "A New Mammalian Fauna from the Fort Union of Southern Montana," Am. Mus. Novitates No. 297, Feb. 2, 1928. "A Collection of Paleocene Mammals from Bear Creek, Montana," *Annals Car. Mus.*, Art. VI, 1929. "Third Contribution to the Fort Union Fauna at Bear Creek, Montana," Am. Mus. Novitates No. 344. March 18, 1929.

Paleocene horizons of New Mexico, but no vertebrate evidence had been produced. The known stratigraphic sequences and lithologic units in the two areas did not match well enough for any positive statements. Therefore a program of detailed stratigraphic and faunal studies of the strata between the Lance formation and the 'Clark Fork beds' seemed desirable and was undertaken by the writer. The chief result is the comprehension that the Paleocene section of N. W. Wyoming contains representatives of all the major mammalian faunal units of the epoch previously known in North America.

IV. FIELD EXPLORATIONS

THIS paper, describing the progress and significance of some of the work in the Paleocene and contiguous sediments in the northern part of Park County, by the Scott Fund Expeditions of 1927, 1928, and 1929, is designed to help furnish a preliminary basis for future work upon the stratigraphy of this rather critical Cretaceous-Tertiary demarcation area. No doubt some alarm is justified at the announcement of a supplement to the already protracted register of essays relating to the Mesozoic-Cenozoic partition, but the material herein contained treats chiefly of observation and has nothing to defend.

The 1927 field season served to familiarize the collectors with the fauna and diagnostic lithologic characters of the Gray Bull in the classic ground near Otto, in the middle of the basin. Even in this frequently-explored territory, the work was rewarded by many fossil specimens [1] and reinforced the impression that the almost unknown lower Paleocene deposits might repay prospecting by divulging yet more interesting information.

Subsequent collections and sediment studies progressed downward from the well-known to the little-known, from the

[1] These publications describe part of the material: Sinclair, W. J., "*Omorhamphus*, a New Flightless Bird from the Lower Eocene of Wyoming," *Proc. Am. Phil. Soc.*, Vol. LXVII, No. 1, 1928. Sinclair, W. J., and Jepsen, G. L, "A Mounted Skeleton of *Palaeonictis*," *Proc. Am. Phil. Soc.*, Vol. LXVIII, No. 3, 1929. Jepsen, G. L., "New Vertebrate Fossils from the Lower Eocene of the Bighorn Basin, Wyoming," *Proc. Am. Phil. Soc.*, Vol. LXIX, No. 4, 1930.

Gray Bull into the Lance, attended by lucky and significant discoveries.

Intensive collecting in the Clark Fork Basin in 1928 established the presence of representatives of the genus *Homogalax* in typical "Sand Coulee" beds. Inasmuch as the apparent absence of the genus from these "beds" had been the basis for differentiating them from the Gray Bull, the separation is no longer valid, and the Gray Bull member is here redefined to include all of the sediments between the "Clark Fork beds" and the Lysite.

Now the lower boundary of the Gray Bull had to be established in an area where no unconformity could be detected. In lieu of any lithologic separation between the Eocene and the Paleocene sediments, the presence of *Eohippus* in the former, a classic distinction, proved still to be the most workable faunal criterion. The boundary was determined as shown upon the sketch map, Fig. 2.

A systematic collection of specimens immediately below this division, and an appraisal of their significance, reinforced the impression that there is an uppermost Paleocene level which is rather distinct faunally from the underlying sequences. It was from this level that the American Museum's "Clark Fork" specimens presumably came, although in most places the horizon lacks physical evidence of unity, being indistinguishable from the sandstone-shale series enclosing it. Localities 1 and 2 on the map, Fig. 2, contributed important new elements to the "Clark Fork" fauna.

Below this topmost Paleocene zone, or typical "Clark Fork," the beds are poorly exposed because the main drainage course of Big Sand Coulee, though usually dry, lies along their strike and has such an abundant supply of sediment from its badland head-tributaries that it is aggrading and not producing fresh dissection.

Farther northeast, however, lower beds are well exposed in limited badlands, and here was discovered a layer which produced a microfauna abundantly along a few yards of exposure, locality No. 3 on the map, Fig. 2, and Plate I, Fig. 2.

PLATE I

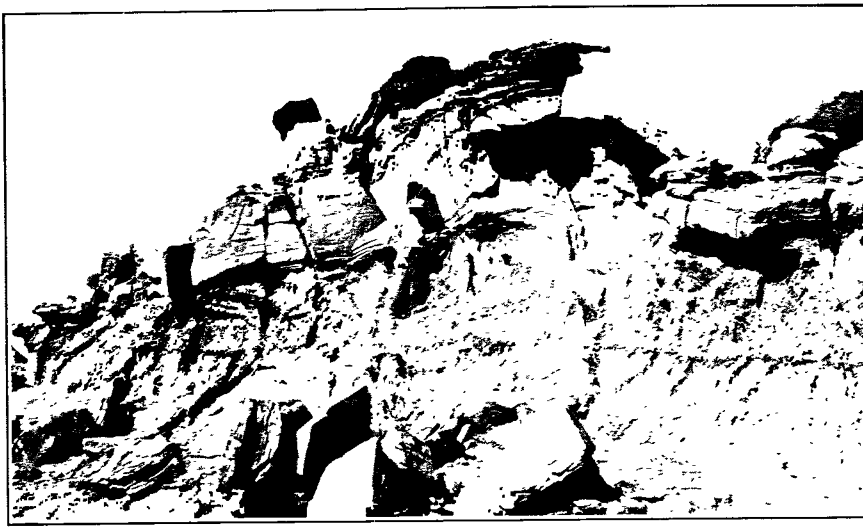


FIG. 1.—Locality No. 6 on map, text fig. 2. Collector picking at base of channel sandstone containing Torrejon genera.

FIG. 2.—"Princeton Quarry" preliminary excavations. Collector is indicating lowest part of fossiliferous level.

Quarrying revealed, among other material, specimens referable to the genera *Carpolestes*, *Leptacodon*, and *Ectypodus*, which show the level to be contemporary in part with the Bear Creek facies of Montana and the Tiffany of Colorado, both Upper Paleocene in age. In the absence of any local geographic name which would be apropos and unconfusing, the excavation will be called "Princeton Quarry."

Just below the horizon of this quarry the beds are very sparingly fossiliferous, rewarding the collector only by a tooth or two for a hard day's prospecting. However, *Clænodon*, locality No. 5 on the map, and *Titanoides*, No. 4 and No. 5, were both found. From here down into what had been mapped as the top of the Lance, there remained for examination several hundred feet of presumably barren sediments. While the collectors were securing hand specimens of an extremely coarse, almost conglomeratic, sandstone in the vicinity marked 6 on the map, they unexpectedly exposed a tooth of *Ptilodus* and a small Insectivore jaw. This chance discovery led to the collection of a Torrejon fauna including *Chriacus*, *Tricentes*, and *Tetraclænodon*, all from the same limited sandstone lens. Plate I, Fig. 1.

Seventy-five feet below this Torrejon level is the top of the massive sandstone (130 feet thick), which the U. S. Geological Survey has included in its "Upper Lance" upon the state map of 1925. A small fauna, composed mostly of Puerco genera such as *Loxolophus*, *Eoconodon*, and *Oxyacodon*, was fortunately discovered in the very base of the sandstone, at position No. 7 on the map, about 200 feet below the Torrejon level. Only twenty feet below the Puerco sandstone, dinosaur bones had weathered out from the shale, and a short distance farther down dinosaur tracks appeared on some of the sandstone surfaces, and yet lower, the collectors secured *?Triceratops* teeth.

The discovery of a Puerco fauna in a sandstone mapped as "Lance," has necessitated a relocation of a portion of the Cretaceous-Tertiary boundary, and the revised mapping is indicated upon Fig. 2.

After the collection of the Puerco fossils, detailed stratigraphic observations were essayed, beginning within the Lance and extending upward through the complete Paleocene section, Puerco, Torrejon, Princeton Quarry level (Tiffany—Bear Creek equivalent), "Clark Fork," and into the lower Eocene Gray Bull. The stratigraphy of these elements is described below, in geologic order.

V. STRATIGRAPHY, SEDIMENTS AND FAUNAS

Lance

The lower part of the Lance was not considered in detail, nor was the base of the formation observed. None of the strata were examined below a coarse white sandstone at the foot of the escarpment east of the Frannie Canal, SW ¼ Sec. 32, T57N, R98W. This sandstone is from 10 to 40 feet thick and though, in eroding, it forms a ledge at the base of the cliff, it nevertheless crumbles readily and becomes rapidly sculptured into fluted banks somewhat in the fashion of the "Hell Creek Beds" described by Brown at the base of the Lance east of Gilbert Creek, Garfield County, Montana.[1] No fossils were collected from this sandstone in Polecat Basin.

For an undetermined distance above this sandstone, but not in excess of 50 feet, the strata are alternating sandstones and shales, with a steady decrease in the amount of sandstones until they occur only as infrequent small lenses in a thick dark shale series with a few yellow-weathering strata. This monotonous shale erodes into rounded hillocks, and disintegrates at the surface into a thick fluffy blanket which must be removed, often to a depth of a foot, before any semblance of bedding is observed. This level contains horned-dinosaur teeth,[2] jaws, maxillæ, and skeletal parts, and fragmentary remains of duck-billed dinosaurs. Fossils of fish abound in the form of scales, teeth, and vertebræ. Small reptile teeth and vertebræ, probably of lizards, were also collected.

[1] Brown, B., "The Hell Creek Beds of the Upper Cretaceous of Montana," *Bull. Am. Mus. Nat. Hist.*, Vol. 23, Art. 33, p. 832, Fig. 6.

[2] Mr. Barnum Brown kindly verified this determination after examining representative material.

Above the dark shale phase, sandstone again becomes a common intercalation for more than 100 feet, to the top of the formation as herein redefined. There are many thin beds of highly carbonaceous material, and plant remains were observed throughout the series, in both sandstone and shale. Small local limestone beds are not infrequent. A highly interesting specimen of a fine-grained sandstone was collected from these beds. It contains many small clay "galls," up to ⅛ of an inch in diameter, and irregular in shape. Quartz is the chief mineral in the rock, and the particles vary from highly angular chips to sub-rounded grains, some of which are "frosted" and mildly pitted. Feldspars, microcline and orthoclase, and plagioclases occur in abundance as moderately rounded and well-weathered particles. Tourmaline is present, as is muscovite and zircon and possibly apatite. Limonite appears in small amounts. Shells of small fresh-water pelecypods and gastropods, and broken pieces of larger invertebrate shells, make up a considerable part of the rock, and endow it with a marly aspect. In washing and separating the constituents of the sample, many brown flakes appeared, and upon closer examination displayed certain characteristics of flexibility, texture, and microscopic cell structure which branded them as organically derived. The puzzle was solved with the finding of complete, but partially collapsed and sediment-filled, chitinous larval insect cases which still retain many of their original properties. Some very small bone fragments and conical teeth of vertebrates, probably reptilian, have also been observed. Carbonized plant remains are abundantly present. Spore cases of some undetermined species of *Chara* similar to *Chara petrolei* [1] were isolated. The prone attitude of the flat shell pieces, probably aided by subsequent compaction, has given the rock a vague "bedding." Evidently the sediment is derived from granite, is partly wind-blown but mostly stream-transported, and deposited in a short-lived pond.

[1] Andreae, von Dr. A., "Ein Beitrag zur Kentniss des Elsasser Tertiars," *Abh. Geol. Specialkarte von Elsass Lothringen*, Band II, Heft. III, p. 162, Taf. V, 1884.

In the eroded basin of Polecat Dome, T57N, R98W, some very large dinosaur bones were seen in both sandstone and shale. Within 30 feet of the top of the Lance, an imperfect obverse record of ripple marks and dinosaur tracks is preserved on the base of a sandstone lens.

Presumably there is no stratigraphic break of great significance in this series, for dinosaur bones were found throughout, up to 20 feet below the overlying basal Fort Union.

The sandstones and shales of the uppermost part of the dinosaur-bearing beds present remarkably quick changes in character and thickness along the general strike (Plate II). In several instances thick sandstone lenses were observed to taper into mere sheets or disappear entirely within a few feet. The characteristic discontinuity and irregularity and large-scale cross bedding of these strata may mask a slight angular unconformity at the top. In places there is no sandstone whatever in the upper 25 feet of shale. But the topmost shale usually has a concretionary habit, and becomes increasingly carbonaceous upward until it is transitional into a dirty coal. This coal appears almost continuously throughout the area examined, at the top of the Lance sediments and just below the massive buff scarp-forming sandstone which Hares includes within the Lance, but which contains a limited fauna of Puerco genera. For this reason the Lance (Cretaceous)—Fort Union (Paleocene) boundary has been relocated as shown on the map, Fig. 2, and this great sandstone member is hereafter designated in this paper as "Puerco sandstone." The surface upon which the coal rests has many features of interest, and the coal itself may tell a story. It lies upon an undulatory surface whose minor irregularities are filled in by the coal, which, in consequence, varies in thickness from a few inches to several feet. Multitudinous small plant-root remains and sediment-filled root cavities extend from the coal vertically downward, sometimes for several feet, into the underlying sediments which vary from soft shale to hard, finegrained sandstone. The minute cementing calcite crystals

PLATE II

FIG. 1.—Looking eastward along general strike of beds in south wall of Polecat Basin, T57N, R98W, Sec. 31, showing Lance-Fort Union contact.

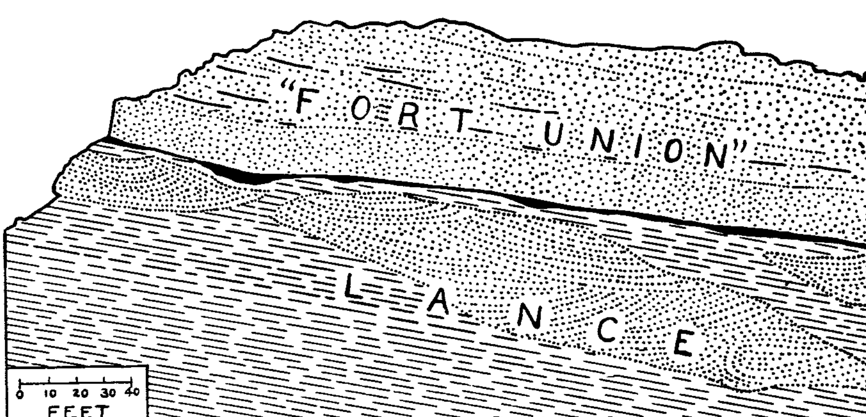


FIG. 2.—Key diagram for picture shown above.

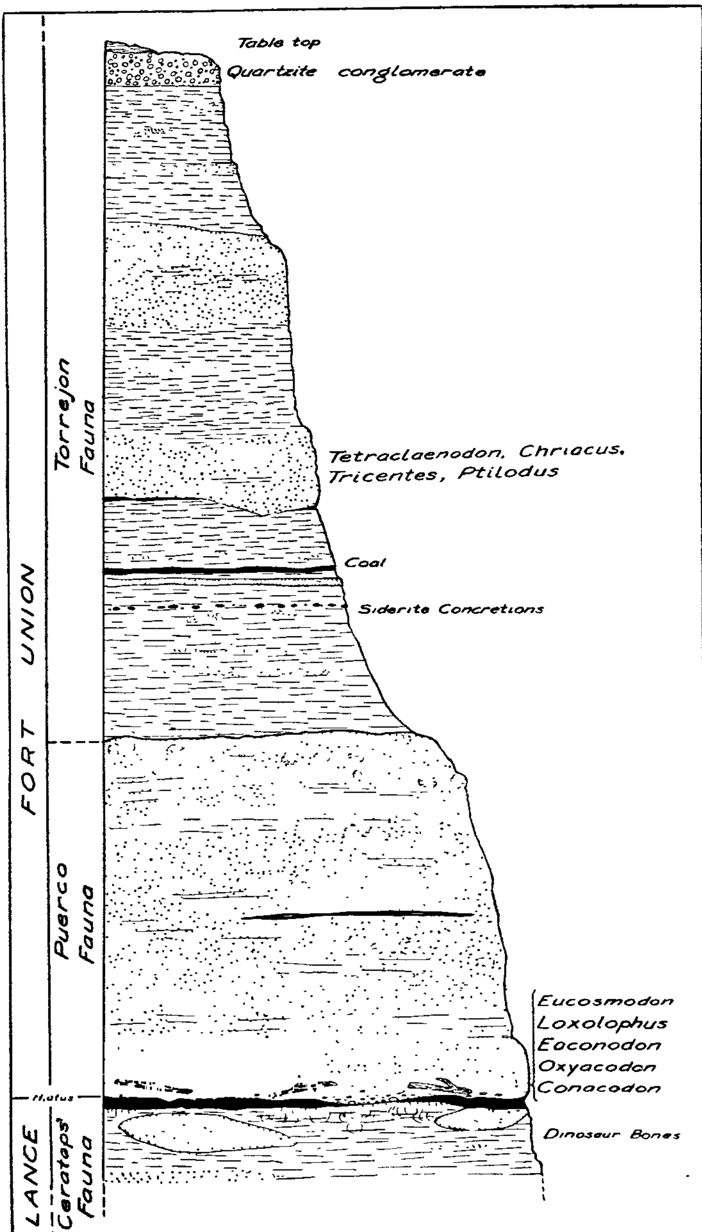


FIG. 3.—Generalized section of southwest wall of Polecat Basin, T57N, R98W, Sec. 31

of one sandstone sample from this level are in parallel orientation, and permit the sandstone to have definite cleavage faces that reflect light as from the surfaces of a broken sand-calcite crystal. This sandstone contains many fragments of plants. The leaf traces are usually flat-lying and broken into small irregular pieces, less than an inch in any dimension.

In a small area where the coal was missing, the usual plant roots nevertheless penetrated the underlying sandy carbonaceous shale, though they were truncated at the top by the overlying coarse sandstone and clay-gall conglomerate which encloses mammalian remains of Puerco type. Sometimes the rootlets can be traced upward into the lowest part of the impure coal itself. An attempt has been made to show some of these phenomena graphically in the diagram, Fig. 3, which illustrates a generalized section of the southwest wall of Polecat Basin in T57N, R98W, Sec. 31. The coal mentioned above is shown in the diagram at the top of the Lance, and the heavy sandstone member above it is the so-called "Puerco sandstone" which will now be described.

"Puerco Sandstone," Base of the "Fort Union"

In many places the soft coal and shale have disintegrated and caved away from under the Puerco Sandstone ledges and left the base of the sandstone intact, preserving excellent obverse records of the surface upon which the sand was deposited. There are occasional widely spaced chann s, and numerous slanted incisions, irregular in cross-section, all filled by sandstone. Patches of regularly disposed undulations may be evidence of ripple marks on top of plant material that was disintegrating and compacting in shallow open water.

Plate III, Fig. 2, shows the impression in the sandstone of a compressed, cycadlike trunk. The camera was pointed directly upward under the sandstone ledge. Fig. 1 on the same plate is a picture, taken in a similar way, of some very puzzling little flattened sandstone pendants from which the underlying coal has disintegrated. These curious structures vary, in general, from a quarter of an inch to an inch in

PLATE III

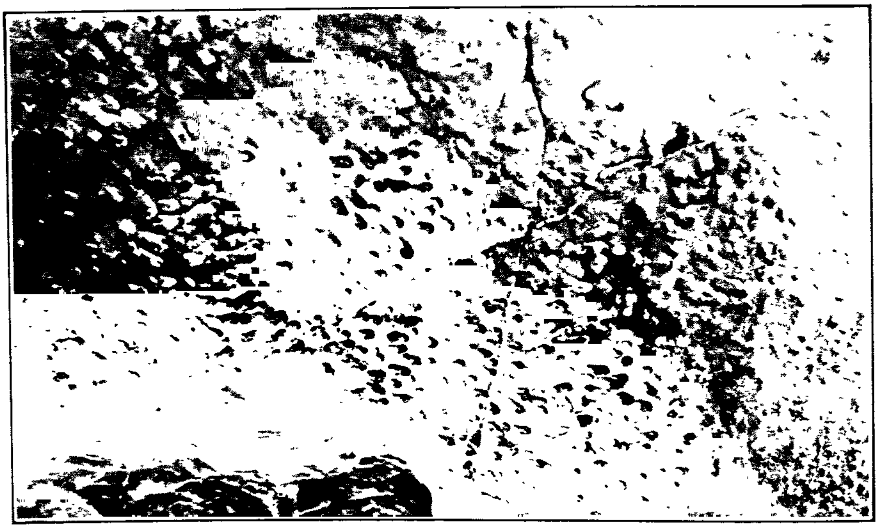


FIG. 1.—Structures interpreted as sand-filled burrows of insect larvæ, base of Puerco sandstone, T57N, R98W, Sec. 31, south wall of Polecat Basin. Camera directed upward under overhanging ledge.

FIG. 2.—Mold of a cycad (?) trunk, in base of Puerco sandstone at contact with Lance coal. Same locality as above, taken under similar circumstances.

length, and are usually about half an inch across and an eighth of an inch thick. At first glance they look like little flat oval pellets, but then it is observed that only one end is free, the other being attached to the sandstone. They are like minute membranous sand-filled bags, flattened. Their surface is smoother than the bottom of the sandstone is elsewhere, and they have a somewhat imperfect orientation, readily seen in the photograph. All of these observations are in accord with the suggestion that the structures may be sand-filled and compressed borings or cases of larval insects.

In sharp contrast to the great variation of the strata immediately under the coal, the first fifteen or twenty feet of the Puerco sandstone seems to be a stratigraphic unit that extends, with but little variation in thickness, throughout the area studied in Polecat Basin in Sections 31 and 32, T57N, R98W; Section 1, T56N, R98W; and Sections 24, 25, and 36, T57N, R99W. It is continuous above with the rest of the sandstone member which has a total thickness of 130 feet, but the upper reaches are separable into "beds" and include thin shale layers. The lowest part of the sandstone varies from moderately coarse to coarse texture, and is composed chiefly of very angular quartz fragments, although some grains are well rounded. Rutile needles penetrate the quartz. Weathered and worn feldspars are present. Magnetite grains are not uncommon. Mica flakes become so abundant in places as to be the chief constituent. Hornblende and garnets and other minerals have been identified in minute quantities. The yellow color of the sandstone is due to an iron oxide coating which increases in prominence downward toward the base until the very contact is nearly everywhere a deep brown.

In the places, previously mentioned, where the coal is not present and the roots underneath have been truncated, the basal sandstone assumes the nature of a torrential debris accumulation, and it is here that the vertebrate remains are most abundant. In some hand specimens of the sandstone, small flattened clay pebbles or galls make up most of the mass.

Often these pebbles are iron-stained and covered with a hard iron coat that remains as a limonitic shell with delicate cross-partitions after the inside material has been removed by natural forces. Massed and distorted pseudomorphic cubes of impure hematite after pyrite crystals occur close to the base. The sandstone itself is of vast variation in grain-size, from fine material to a conglomerate, and is more strongly cemented with calcite than the normal basal phase. Quartz fragments, only slightly rounded, and fresh feldspar particles with dimensions up to an eighth of an inch are not uncommon. Well rounded quartzite pebbles also occur, frequently with a diameter of an inch. One black quartzite cobble was found in the form of half a flattened ovoid with a maximum dimension of 5½ inches. The rock was split lengthwise before deposition at this spot, and the other half was not found. The broken surface is rugose and makes angular corners with the well-rounded outside portion. Upon further breakage, three fracture cleavage systems were revealed. To the greatest axis they are parallel, normal, and at about 45°.

Invertebrates were not found in this basal phase of the sandstone. Vertebrate relics, however, abound in the form of fish scales and vertebræ, crocodile scutes and teeth, and other reptile teeth and bones. Some fragmentary mammal jaws, maxillæ, and single teeth rewarded diligent prospecting and quarrying. Most of the bones and teeth were found within a few inches of the base of the sandstone. The matrix is so much harder than the bones that their excavation is a delicate process. They are sometimes filled with impure calcite, and show little or no water wear, though they are often broken. The following forms have been determined:

MULTITUBERCULATA

Ptilodontidæ

Eucosmodon gratus, new species, description on page 499, Plate IV, Fig. 8.

CREODONTA

Oxyclænidæ

Loxolophus nordicus, new species, description on page 501, Plate IV, Fig. 1.

Loxolophus sp. Plate IV, Fig. 9.

Triisodontidæ

Eoconodon sp. Plate IV, Fig. 5.

Mioclænidæ

Oxyacodon sp. Plate IV, Figs. 2, 3.

Periptychidæ

Conacodon sp.

Loxolophus, *Eoconodon*, *Oxyacodon*, and *Conacodon* are typical Puerco genera, heretofore found only in the limited type area of the Puerco zone of New Mexico.

Irregular masses of coal with diameters up to two feet are included in the lower five feet of the chaotic phase of the basal sandstone, and plant remains are abundant. The variation in their occurrence from the contact upward was noted as a matter of some interest, for at the very base, in the clay-gall conglomerate, they are not plentiful, and yet a few inches higher they abound in the form of broken and compressed branches and nuts, fruits, or seeds. Just a few inches above this level there are fewer plant traces, but sometimes huge flat remains, or the cavities which they once occupied, can be traced for fifteen and even twenty feet, as represented in Fig. 3, with a width up to a foot or eighteen inches. They do not lie flat, but seem to be irregularly disposed, and penetrate the sandstone at low angles.

Above this basal unit, the Puerco sandstone varies only slightly in texture and color, and encloses thin lenses of sandy shale. A few inches were observed to be composed largely of small angular fragments of carbonized wood, giving the sandstone a "freckled" appearance along a fresh break. Toward the top there are many concretions which erode out as spheres and "logs" and weather to a deep brown color.

Favorable circumstances of erosion have exposed some superb examples of cross-bedding, foreset, and topset beds in the upper part of the sandstone on the ridge south of Polecat Dome. Fractures divide the massive sandstone into colossal blocks which align the smaller stream tributaries and assist erosion into a somewhat rounded, but nevertheless angulate, design.

The uppermost surface has a relief variation of several feet. Imperfect ripple-marks and some plant remains were observed at the very top where the sandstone assumes a deep purplish-brown color and is of an unusual hardness due to oxides of iron.

Turtle plastron and carapace fragments, reptile teeth, and fish scales were seen to occur sparingly throughout the Puerco sandstone.

The persistence of this stratigraphic unit, and its habit of forming a pine-clad ridge, favor its being designated by a name for future handling; but, with great exercise of forbearance, no name is here proposed.

"Torrejon"

Shale with variable carbonaceous content overlies the Puerco sandstone to a thickness fluctuating from a few inches to several feet. It is part of a series of somber intercalated shales and sandstones which appear barren of vertebrate remains for about 72 feet.

At a distance which was not measured above the "Puerco sandstone," in T57N, R99W, Section 25, there appears a shale level which contains many flat ovoid siderite concretions, with a coating of hematite and limonite (see Fig. 3). The carbonate-center portions vary in diameter from a fraction of an inch to four inches. Some may be larger. The oxidized shell is usually less than a quarter of an inch thick, and assumes the pitted or wrinkled and gathered surface characteristic of oxidizing iron carbonate. Displacement of shale laminæ above or below the concretions could not be detected.

Fifty feet above the Puerco sandstone occurs a bed about

a foot thick, which was mistaken in the field for an andesitic ash because of its megascopic properties such as weight, color, texture, and "feel." It weathers white, and has a grayish pink color upon fresh surfaces. But a microscopic examination shows that quartz fragments are the chief, and almost the only, constituent. They are mostly very angular, sometimes flaky, but a very few are slightly rounded. Some particles are pitted, and many, even of the smallest size, are frosted. Perfect little quartz crystals with prism faces and pyramidal terminations are present, with a length up to .12 mm. Occasionally one side of a crystal will be frosted. Mica foils and good, but exceedingly small, rhombic-dodecahedral crystals of garnet can also be observed. The following analysis by Professor A. H. Phillips is of an average sample collected in the SE ¼ Sec. 31, T57N, R98W:

SiO_2	93.53
Al_2O_3	4.93
Fe_2O_3	.06
MgO	None
CaO	.19
H_2O	1.50
	100.21

Fossils of leaf and twig fragments abound in every exposure of the sandstone which was examined. All of the evidence of composition, grain size and shape, "pitting," and "frosting," conforms to the opinion that the material was transported by wind.

A few feet above this fine silicious sandstone, in the little valley that is drained by the south branch of Polecat Creek, there is a coal bed about two feet thick (see Fig. 3 "Coal"). It has been mined, but the workings are now abandoned. Coal in a similar position was at one time mined in the trough of the adjoining syncline, where it plunges into the face of Polecat Bench, W ½ Sec. 24, T57N, R99W. Somber shales about 15 feet thick are on top of the coal, and they in turn are overlain by the sandstone, from 9 to 31 feet thick, which contains mammalian genera of Torrejon age. Text Fig. 3 and

Plate I, Fig. 1, both show the sandstone and illustrate its relation to the underlying shales. The collector, Plate I, Fig. 1, is shown picking at the very base of the sandstone. Although the weathered surface of the sandstone is soft and crumbly, freshly exposed portions are tough and resistant.

Impressive characters of this sandstone are its slight horizontal extent, its irregular truncation of the underlying shales, its massive distorted bedding, and the great variation in thickness. At the base, the sandstone is white in color and contains many flattened, blue clay-galls; but the white color grades upward into a brown phase which has very few clay inclusions. Calcite is the cementing material. Grain size commonly varies from the microscopic to about 3/16 of an inch in diameter. Most of the particles are angular, pitted, and frosted, but a few small ones are very well rounded and highly polished. Quartz fragments are the most abundant constituent and sometimes they contain rutile needles. Tiny quartz crystals, terminated on both ends by pyramids, are common. Other materials that have been determined are orthoclase, plagioclase, biotite, garnet, magnetite, gypsum (in crystalline masses), limonite, quartzite, chert, and clay. A small hollow sphere was observed, lined with quartz crystals in parallel orientation.

Invertebrates were not found in the sandstone. Plant traces are rare in it. Reptile teeth and skeletal parts, turtle remains, and fish scales were seen. The small mammalian collection includes the following forms:

MULTITUBERCULATA

Ptilodontidæ

Ptilodus sp. Plate IV, Fig. 10.

INSECTIVORA

Pantolestidæ

Palæosinopa sp. Plate V, Figs. 7, 8.

CREODONTA

Oxyclænidæ

Chriacus sp. Plate IV, Fig. 4.
Tricentes sp.

Artocyonidæ

Clænodon sp.

CONDYLARTHRA

Phenacodontidæ

Tetraclænodon sp.

?

?

Plesiolestes problematicus, new genus and species described on page 505, Plate IV, Figs. 6, 7.

This appears to be a Torrejon assemblage, in which *Clænodon*, *Tetraclænodon*, *Chriacus*, and *Ptilodus* are the commonest elements.

Most of the teeth in such delicate little jaws as *Plesiolestes* are smaller than the individual particles in the coarse matrix, and yet the fossils are not crushed. They are fragmentary, however, and the broken edges are sometimes rounded and smooth, as are some of the single tooth roots and enamel particles. Other pieces have sharp, unworn fracture surfaces. Sand fills the cavities in the broken bones.

Sedimentary layers above the Torrejon sandstone, up to the top of Polecat Bench, Fig. 3, are mostly somber shales and sandstones, and resemble the underlying material. On the south side of the basin of Polecat Dome, the distance from the sandstone to the table top varies from about 70 feet to 120 feet. None of the strata extend very far horizontally, and hence any discussion of a detailed stratigraphic sequence is inaccurate a few feet from where it is measured.

Sections north of the two faults shown upon the map, Fig. 2, in the southwest corner of T57N, R98W, show almost no sandstone up to the table top, but in one place in the down-faulted block or graben between the two faults there appears a massive brown sandstone forty feet thick.

In this area, the edge of the table top is formed of sandstone and quartzite conglomerates, whose thickness could not be determined accurately, though it exceeds 15 feet in places (Fig. 3). Most of the pebbles have diameters of less than an inch. They are somewhat assorted into lenses of irregular size and attitude through the coarse sandstone. Although most of the smaller grains are quartz, the larger ones are black, gray, and green quartzite, giving the mass a grayish color which is often tinted a light buff by a limonite veneer. A small amount of calcite weakly cements the grains. They show various stages of wear. Some of the quartz fragments and minute crystals are flat-faced and sharp-angled. Others are well-rounded and polished. The larger pebbles are moderately worn, but never completely rounded.

This conglomerate could not be certainly located south of the main Polecat Dome fault, in the column of the upthrown side, and was traced only a short distance north of the graben. It should not be confused with the gravel which appears in intermittent patches on the top of Polecat Bench.

Polecat Bench, an elevated remnant of a former general erosion level, hinders any attempt at accurately measuring the stratigraphic column; and although the sediments are fairly well exposed along the east and southeast margin of the bench, no satisfactory calculation has been made of the thickness of the alternating sandstone and somber shales between the quartzite conglomerate and the first horizon above it which yields a characteristic and abundant fauna, indicated by circle No. 3 on the map. However, a very few vertebrate fossils of Torrejon type were collected from the breaks along the side of Polecat Bench, from Section 2 in T56N, R99W, southwestward to Section 18, and along the northwest side of the bench, presumably at about the same levels, in the southwest corner of T57N, R99W, and the southeast part of T57N, R100W, circles Nos. 4 and 5 on the map. *Champsosaurus* remains appear the most abundantly, but the list also includes *Clænodon corrugatus* Cope, *Clænodon* sp., *Titanoides gidleyi*, new species, *Tetraclænodon*, and *Plesiadapis*. Petrified logs

and upright stumps were noted near the road that ascends Polecat Bench north of Powell. Leaf-bearing shales, impure coals, and carbonaceous beds with much associated gypsum occur commonly in the lower part, and, with decreasing frequency, toward the top of this series. Yellow beds of almost pure limestone, several inches thick, are not uncommon, and form small ledges in the badlands. This limestone shows no organic structure itself, but in places it invests and fills many invertebrate shells, which preserve even their most delicate surface markings and retain their gray color. The lime of the shells appears to be quite independent of the limestone matrix. The latter is completely amorphous, with no indication of crystallization. Upon treatment with HCl it dissolves almost entirely, but leaves a small residue of clay particles and extremely fine angular fragments of silica. The yellow color is due to a small iron oxide content.

The following molluscs were collected from the sediments between the Torrejon sandstone (Plate I, Fig. 1, Locality 6 on the map, Text Fig. 2) containing *Tetraclænodon*, *Chriacus*, *Tricentes*, and *Ptilodus*, and the level which yields Tiffany-Bear Creek mammals (Locality 3 on the map, Fig. 2):

Unio danæ Meek and Hayden?
* *Unio wasatchensis* Cockerell?
Unio sp.
Vivipara retusa (Meek and Hayden)
* *Vivipara paludinæiformis* (Hall)
* *Vivipara*, new species
Lioplax nebrascensis (Meek and Hayden)
Lioplax nebrascensis, var. producta (White)
Lioplax limnæiformis (Meek and Hayden)
Goniobasis tenuicarinata (Meek and Hayden)

All of these are aquatic species (stream, pond, or swamp forms) and, with the exception of the three indicated (*), have been reported previously from the Fort Union. Of the three exceptions, *Unio wasatchensis* occurs in the Gray Bull, *Vivipara paludinæiformis* comes from the "Knight formation" of the Evanston area, and the new species of *Vivipara* is as yet undescribed.

The determinations and systematic arrangements are by Mr. L. S. Russell, and have been supplied by him from his unpublished data.

Tiffany-Bear Creek Faunal Equivalent

Although no pronounced stratigraphic break was observed above the Torrejon sandstone, there appears to be a rather sudden vertebrate faunal change near the middle of T57N, R100W, No. 3 on the map, Princeton Quarry level. It is true, however, that the new genera from the quarry are all small and might easily evade detection if they are sparingly present below this one horizon.

In or near the quarry, Plate I, Fig. 2, the following forms have been found:

Multituberculata

Ptilodontidæ

Microcosmodon conus, new genus and species, description on page 508, Plate VII, Figs. 3, 4, 5.
Ectypodus sp.
Parectypodus sp.
Ptilodus sp. Plate IX, Fig. 6.

Insectivora

Leptictidæ

Diacodon minutus, new species, description on page 511, Plate IX, Figs. 7, 8, 9.
Leptacodon packi, new species, description on page 510, Plate VIII, Figs. 4, 5.
Litolestes ignotus, new genus and species, description on page 513, Plate V, Figs. 1, 2.

?Primates

Plesiadapidæ

Plesiadapis fodinatus, new species, description on page 516, Plate V, Figs. 3, 4, 5, 6.
Phenacolemur pagei, new species, description on page 514, Plate VII, Figs. 1, 2.

CONDYLARTHRA

Phenacodontidæ

Phenacodus sp.
Ectocion sp.

?ARTIODACTYLA

?Dichobunidæ

Phenacodaptes sabulosus, new genus and species, description on page 517, Plate IX, Figs. 1–5.

INCERTAE SEDIS

Carpolestes dubius, new species, description on page 520, Plate VIII, Figs. 1, 2, 3.

The fine gray sandstone matrix investing the bones is composed chiefly of small angular fragments of quartz with bright clean surfaces, and impalpable clay. Gypsum particles and mica flakes appear in smaller quantities, and a few fibrous aggregates of tremolite have been observed in carefully disintegrated and washed samples. A very small amount of calcite cements the particles together, but the rock owes its hardness and toughness more to grain interlocking than to this calcite or to the small percentage of iron oxide that is present.

A layer of harder and coarser sandstone is immediately below the fossiliferous level, but upward it grades insensibly into a shale.

The bones, dissociated and somewhat broken and water-worn, are not confined to a layer, but are dispersed through a vertical thickness of about two feet. Most of them, as well as the teeth, are now black.

THE "CLARK FORK BEDS."

Several hundred feet of somber clays, sandstones, thin limestones, and occasional leafy layers associated with gypsum, overlie the Tiffany-Bear Creek horizon. The fossil yield is scanty, but includes *Phenacodus*, *Ectocion*, and *Dissacus*. It is not until the top of the Paleocene is ap-

proached that a sizable fauna again appears, and this "Clark Fork" proper has the following forms:

	Princeton	*A. M. N. H.*
MULTITUBERCULATA		
Ptilodontidæ		
Parectypodus sp.	X	
INSECTIVORA		
Apheliscidæ		
Apheliscus insidiosus (Cope)		X
EDENTATA		
Metacheiromyidæ		
Palæanodon parvulus Matthew		X
?PRIMATES		
Plesiadapidæ		
Plesiadapis dubius Matthew		X
P. cookei, new species, description on page 525, Plate X, Figs. 1–2	X	
CARNIVORA		
Oxyclænidæ		
Thryptacodon sp.		X
Mesonychidæ		
Dissacus prænuntius Matthew	X	X
Oxyænidæ		
Oxyæna sp.		X
Oxyæna æquidens Matthew		X
Dipsalidictis platypus Matthew		X
Dipsalodon matthewi, new genus and species, description on page 524, Plate X, Figs. 8, 9	X	
Ambloctonus priscus Matthew		X
Miacidae		
Didymictis protenus leptomylus Cope		X

CONDYLARTHRA		
Hyopsodontidæ		
Haplomylus speirianus (Cope)		X
Phenacodus primævus Cope	X	X
Phenacodus intermedius Granger		X
Ectocion osbornianum Cope		X
Ectocion ralstonensis Granger	X	X
Ectocion parvus Granger		X
Meniscotheriidæ		
Meniscotherium (?) *priscum* Granger		X
AMBLYPODA		
Probathyopsis præcursor Simpson		X
P. sp.	X	
Coryphodon (species undetermined)	X	X
TILLODONTIA		
Esthonyx sp.	X	
INCERTAE SEDIS		
Corpolestes dubius, new species	X	

These 'Clark Fork' molluscs have been identified by Mr. L. S. Russell:

Unio danæ Meek and Hayden?
Oreohelix spp.

A few of these genera are also found, as noted above, in and around the quarry. Future work may fill in so many gaps that this faunal division will be untenable, and the two levels will be found too similar to separate.

Some of the beds yielding the "Clark Fork" fauna are red-banded, and hence the color criterion, formerly used for differentiating the Paleocene from the Eocene in the Bighorn Basin, must be cashiered.

Paleocene—Eocene Boundary

A faunal basis for dividing the Paleocene and the Eocene sediments was necessarily employed after the failure of color-

banding to serve as a distinction, and because the angular unconformity at the margins of the basin where the "Gray Bull beds" truncate the underlying "Clark Fork beds" does not extend to the central reaches of the basin. And so, in this conformable series, the lowest level containing *Eohippus* remains was plotted as the boundary (Fig. 2), locally correcting the contact line mapped by the U. S. Geological Survey.

Several of the faunal criteria which have been used previously in separating the Paleocene from the Eocene have proved fallacious or doubtfully applicable. The list of forms which were supposedly not present in the American Paleocene, but which materialize suddenly in the lowest Eocene, is constantly dwindling. From it *Champsosaurus* must now be deleted, because remains of this genus were discovered repeatedly above the lowest occurrences of *Eohippus*.

If the "Princeton Quarry" level genus *Phenacodaptes*, herein described, proves to belong to the Artiodactyla, then the appearance of this order antedates the beginning of the Eocene epoch. Edentates, represented by *Palæanodon*, and Tillodonts of the genus *Esthonyx*, are in "Clark Fork" sediments as well as in the Gray Bull "beds." *Plesiadapis* likewise occurs in both horizons, and so does the multituberculate *Parectypodus*. The evidence now at hand indicates that two orders, Perissodactyla and Rodentia, and one Primate family, the Adapidæ, are the only large groups of mammals which appear suddenly in the Eocene assemblages, unheralded by ancestors in the Paleocene collections.

There are only two "Clark Fork" genera, *Carpolestes* and *Dipsalodon*, which have not been collected also from the Gray Bull.

The Gray Bull "beds" have already been redefined to include the "Sand Coulee beds," inasmuch as the genus *Homogalax*, whose supposed absence from the latter was the basis of distinction, has been found in typical "Sand Coulee beds." Only one mammalian genus, embracing two species, *Neoliotomus ultimus* (Granger) and *N. conventus* Jepsen, has so

far been found exclusively confined to the lowest Gray Bull ("Sand Coulee beds"). Every other species of the "Sand Coulee" presents itself in the Gray Bull or higher zones. Possibly the varieties within the species are more primitive in the lower than in the upper levels of the Gray Bull, as others have suggested; but such a situation is not unique and cannot be a basis for division.

Invertebrate lists, through the courtesy of Mr. L. S. Russell, may be here introduced as testimony to the essential unity of the "Sand Coulee"—Gray Bull sequence:

	"Sand Coulee"	*Gray Bull*
Unio clinopisthus White		X
Unio sp.	X	
Helicina evanstonensis (White)	X	X
Vivipara leidyi (Meek and Hayden)?	X	
Vivipara wyomingensis Meek	X	
Vivipara, new species		X
Goniobasis carteri Conrad	X	X
Planorbis, new species		X
Physa bridgerensis Meek		X
Physa pleromatis White		X
Physa sp.	X	
Polygyra veternior (Cockerell)	X	X
Gastrodonta coryphodontis Cockerell	X	
Oreohelix megarche Cockerell and Henderson	X	X
Oreohelix grangeri Cockerell and Henderson	X	X
Oreohelix spp.	X	
Gonyodiscus ralstonensis (Cockerell)	X	X
Grangerella sinclairi (Cockerell)	X	X
Grangerella megastoma Cockerell	X	
Protoboysia complicata Cockerell	X	

Regarding their significance, Mr. Russell has kindly supplied the following note:

"The species of this Lower Eocene fauna are, with few exceptions, either confined to these particular beds or are known elsewhere only from Eocene strata. As to habitat, almost three-quarters of the genera represented are terrestrial; and if species and individual specimens are considered, the aquatic element becomes almost insignificant."

This is in marked contrast with the Fort Union assemblage listed on page 489.

VI. STRATIGRAPHIC CORRELATIONS AND CONCLUSIONS

This paper does not attempt any detailed discussion of the stratigraphic relationships of the gratifyingly complete Paleocene section of the Bighorn Basin with correlative horizons elsewhere. In most of the "Fort Union" areas of Montana, Wyoming, and the Dakotas, where the strata have been well defined in lithologic terms, the faunas are scantily known; and, conversely, if the vertebrates have been described biologically, their occurrences are incompletely correlated with the data of stratigraphy.

Thom and Dobbin have constructed apposition charts of the Cretaceous-Tertiary transitional beds of Montana and the Dakotas, describing the following "members"—the Hell Creek and the Tullock members of the Lance, the Lebo and the Tongue River members of the Fort Union, and the Sentinel Butte member and the Ulm coal group of the Wasatch. None of these are at present recognizable in the Bighorn Basin.

Osborn, Matthew, Simpson, and others have presented Paleocene correlation tables, based upon vertebrate evidence. Some of the information with which they worked can now be confirmed and extended by the addition of a few new facts about the "Fort Union."

In parts of northwestern Wyoming, sediments which contain dinosaur bones and are considered Lance in age are directly, though disconformably, overlain by strata yielding Puerco genera. At present the Puerco is regarded as being within the Fort Union for reasons which have been cited, without, however, evaluating the suggestions which would question this procedure.

The "Fort Union" of the Bighorn Basin may not be exactly isochronous with the Fort Union of other localities, but it

contains genera which are held to be diagnostic of these horizons:

Clark Fork (type locality)
Tiffany-Bear Creek
Torrejon
Puerco

The Puerco fauna is too limited to permit more than a general correlation with the only other known occurrence, that of the type area in New Mexico.

The Wyoming Torrejon and Tiffany-Bear Creek levels cannot be closely compared with the approximately contemporary columns of Montana, New Mexico, and Colorado, because there is no reason to believe that the thicknesses approach similarity in the various regions even though the vertebrates of certain levels appear coeval. Much careful exploration for new facts, and critical restudy of already established data, are needed.

Titanoides appears above the Torrejon sandstone and below the lowest occurrence of *Coryphodon*, at localities Nos. 4 and 5 on the map, Fig. 2. *Pantolambda*, a New Mexico and Montana genus, has not yet been found in the basin, but its absence is perhaps only a fortuity of collecting. *Pantolambda–Titanoides–Coryphodon* may be a direct or a kindred descendency. *Titanoides* has many characters, even size, intermediate between the other two. Dr. Thom states [1] that the type of *T. primævus* is, in his opinion, from the lower part of the Sentinel Butte shale; but this is not considered sufficient for correlating the Sentinel Butte with the *Titanoides* level herein described, inasmuch as the two species of *Titanoides* are not identical, and the time range of the genus is still unknown.

Present information makes impossible a statement of exactly where the Bear Creek level finds its closest equivalent in the Wyoming sequence, even though it is in the same general depositional basin. *Carpolestes*, a unique and specialized form, *Phenacodus*, and *Plesiadapis* are common to the Bear Creek assemblage, the Princeton Quarry level, and the

[1] Unpublished communication.

"Clark Fork beds." *Leptacodon* occurs in the Bear Creek and the Princeton Quarry collections, and *Dissacus* and *Thryptacodon* have been found at Bear Creek and in the "Clark Fork." Despite these faunal interconnections and cross correlations, the Bear Creek facies seems more nearly equivalent to that of the Princeton Quarry level, even in the common absence of large forms, than to the Clark Fork fauna. Since the Clark Fork horizon is superposed above the quarry level, a continued apposition of Bear Creek (or Tiffany) and Clark Fork may be a questionable practice.

A chart might be synthesized to show the correlations which the new discoveries indicate, but it is omitted from this report in order to avoid any crystallization of ideas regarding the faunal assemblages.

To Simpson's summarized correlations of European [1] and American Paleocene faunas one bit of information can be added. *Liotomus* of the Thanetian stage is closely related to *Neoliotomus* of the lower Gray Bull. Possibly the two are congeneric.

Evidence that the faunal change from the "Clark Fork" to the Gray Bull is slighter than was heretofore supposed has been reviewed. This comprehension, and the fact that the Mongolian Paleocene Gashato yields a fauna which is unique and not directly ancestral to the Eocene of this country, might lead one to the belief that most of the Gray Bull genera are autochthonous.

Between the Lance and the "Puerco" series in the basin, there is a time gap or hiatus which, if the physical evidence can be trusted, is the only break of note within the Cretaceous-Tertiary sequence of the basin. This observation, and the fact that this disconformity marks the greatest faunal separation in the stratigraphic column of the area, favor the placing of the Cretaceous-Tertiary boundary between the Lance and the Paleocene.

However, it may be pointed out, a judicious selection of data can "prove" two opposed conclusions with equal

[1] Simpson, G. G., "Paleocene and Lower Eocene Mammals of Europe," *Amer. Mus. Novitates*, No. 354, 1929.

facility in discussing any phase of a conception so involved as a universal dividing line between two major geologic time "blocks." If it is desired to demonstrate that the Cretaceous-Tertiary boundary should lie between any two arbitrarily selected formations in the critical precinct, there is stratigraphic evidence to prove the contention; for, in various areas, unconformities are locally present between every two formations from the Pierre to the Wasatch. And, conversely, if the data are plucked so that unconformities are eliminated, the whole series is a gradual transition and the ultimate satisfactory division is a project to be conjured in the laboratory or draughting room.

VII. SYSTEMATIC DESCRIPTIONS OF NEW FORMS

A. Puerco

MULTITUBERCULATA

Ptilodontidæ

Eucosmodon gratus, new species.

Plate IV, Fig. 8

Holotype.—Princeton No. 13373, incomplete left ramus with M_2, and alveoli of I, P_4 and M_1.

Horizon and Locality.—Puerco faunal level of Basal Paleocene, southwest side of Polecat Dome, T57N, R98E, Sec. 31, Park County, Wyoming. Locality No. 7 on map, text fig. 2.

Characters.—The alveolus of the incisor shows that the tooth was greatly compressed and extended back under the molars. In cross section, the outline of the external or labial side was gently convex and the lingual side was nearly straight, but had a shallow groove near the front edge.

In examining the alveoli of P_4, nothing appears strikingly different from the similar alveoli of other members of the genus.

Extreme delicacy prohibits thorough excavation of the matrix in the alveoli of M_1, but the length of the tooth can be approximated.

Though worn, M_2 apparently has the cusp formula characteristic of *Ptilodus* and *Parectypodus:* four external and two internal cusps.

But this new species has some unique shape-characters of M_2. The front edge of the tooth is nearly straight, having almost no concavity like that observed in *Parectypodus*, and the anterior corners are angular. The internal cusp row is about two-thirds the length of the external row. A deep notch indents the enamel upon the posterior border of the posterior internal cusp.

MEASUREMENTS

		mm.
I	height (alveolus)	2.7
	width (alveolus)	1.1
M_1	length (alveolus)	2.9
M_2	length external row	2.3
	internal row	1.7
	width	1.9

An isolated incisor fragment, Princeton No. 13374, found close to the holotype may belong to the same species, but it is a bit larger than can be accommodated by the alveolus of the type. This incisor shows enamel upon the anterior parts of both sides, and is shaped like that conjectured for the type. Its wear is typical of many *Eucosmodon* specimens.

Even though the fragmentary nature of the holotype detracts from its value as a type, it is described for the purpose of placing its characters in availability, facilitating future comparisons, and making the record of the fauna of the level complete.

PLATE IV

FIG. 1.—*Loxolophus nordicus*. Holotype, new species, Princeton No. 13285, right M^{1-3}, Crown view, × 4.

FIG. 2.—*Oxyacodon* sp. Princeton No. 13338, left M_{1-3}, crown view, × 4.

FIG. 3.—Same specimen, internal view, × 4.

FIG. 4.—*Chriacus* sp. Princeton No. 13288, left upper molar, crown view, × 3/2.

FIG. 5.—*Eoconodon* sp. Princeton No. 13290, left lower molar, crown view, × 2.

FIG. 6.—*Plesiolestes problematicus*. Holotype and paratype, new genus and species, composite drawing, Princeton Nos. 13291, 13292. Right lower dentition, crown view, × 4.

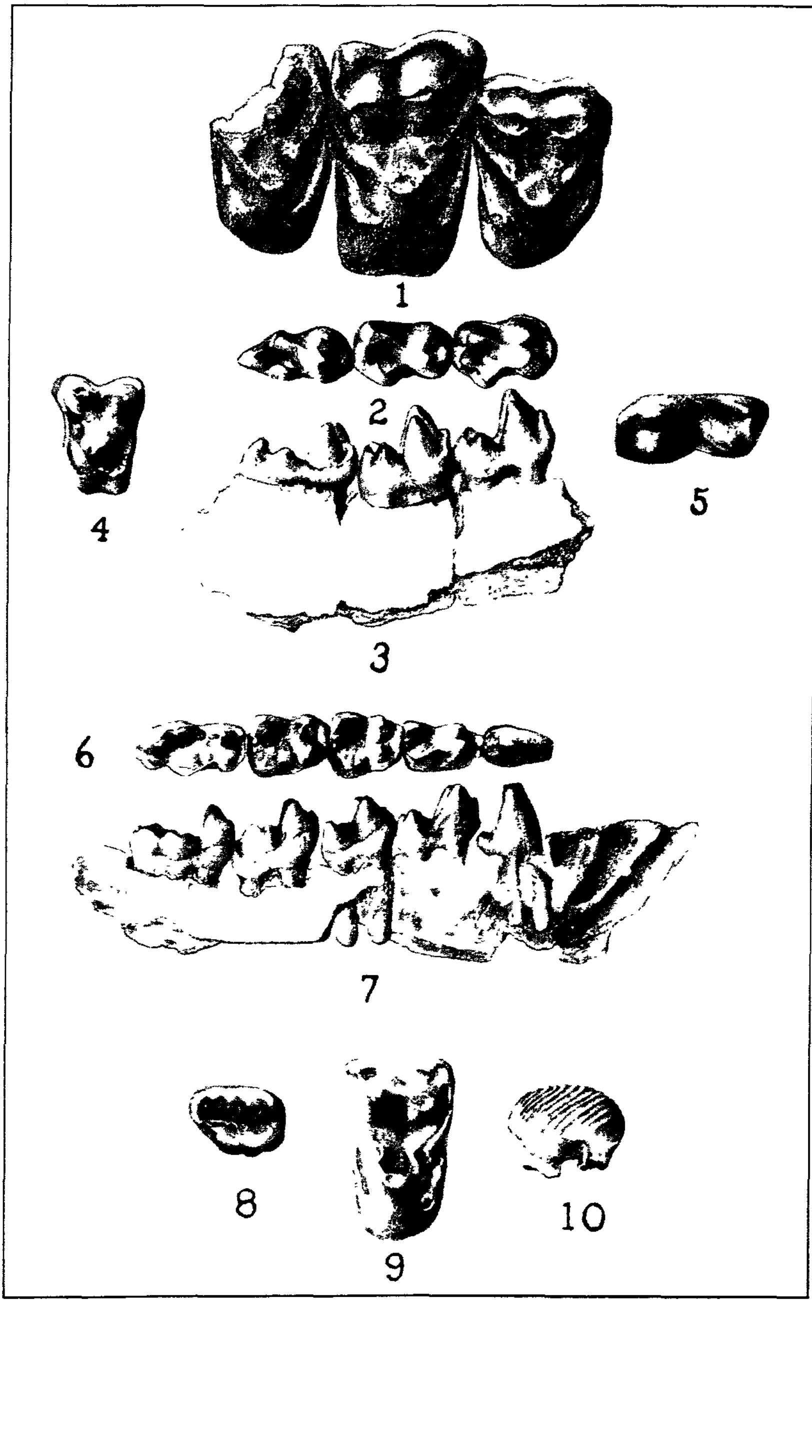
FIG. 7.—Same specimens, external view, × 4.

FIG. 8.—*Eucosmodon gratus*. Holotype, new species, Princeton No. 13373. Left M_2, crown view, × 5.

FIG. 9.—*Loxolophus* sp. Princeton No. 13340. Left upper second molar, crown view, × 4.

FIG. 10.—*Ptilodus* sp. Princeton No. 13361. Lower left sectorial, external view, × 2.

PLATE IV



CREODONTA

Oxyclænidæ

Loxolophus nordicus, new species.

Plate IV, Fig. 1

HOLOTYPE.—Princeton No. 13285, right maxilla with M^{1-3}.

HORIZON AND LOCALITY.—Base of "Puerco sandstone," southwest wall of Polecat Basin, T57N, R98W, Sec. 31, Park County, Wyoming, locality No. 7 on map, text fig. 2.

CHARACTERS.—In nearly all respects the characters of the molars of this species are very similar to those of *L. hyattianus* (Cope). The chief differences are of proportion and minor cusp variation. M^1 is subquadrate in outline. The protocone is very large and is closely joined to the conules; the paracone and the metacone are subequal in size, and are dominantly cone-shaped, but are modified by anterior and posterior keels. Possibly a parastyle was present, but it is now broken from the specimen. The external cirgulum enlarges slightly in the mesostyle region, and the metastyle is distinct. The posterior cingulum supports a little hypocone, and the anterior cingulum bulges in the ectocone position. A ridge extends from the paraconule to the parastyle, and a similar one connects the metaconule with the metastyle. An unusual, small cuspule appears at the lingual base of the paracone.

M^2 is much larger than M^1, and has a similar cuspidation with but a few differences. The parastyle is distinct, the mesostyle is a tiny enlargement of the external cingulum, and the metastyle is merely a diminishing ridge from the metacone. The external cingulum shows the same depression in its posterior part that is observed on *L. hyattianus*, just anterior to the metastyle. M^2 lacks the accessory cuspule at the base of the paracone which was noted on M^1, and M^2 also fails to show the hypocone and ectocone upon the posterior and anterior cingula, respectively. M^3 is less quadrate and more triangular in outline than M^{1-2}.

The small part of the maxillary bone which is preserved agrees in all observable details, such as shape, direction, and origin-area of the malar process, with *L. hyattianus*.

MEASUREMENTS

	mm.
M^{1-3}	18.9
Transverse diameters	
M^1	6.8
M^2	8.3
M^3	7.0

Anterior-posterior diameters	
M^1	4.8
M^2	5.1
M^3	4.4

Loxolophus sp.

Plate IV, Fig. 9

Princeton No. 13340, left upper second molar.

HORIZON AND LOCALITY.—Same as in the preceding.

The cuspidation of this tooth is in all essential details like that of the corresponding tooth of *Loxolophus nordicus*, Princeton No. 13285. The proportions of this specimen are, however, somewhat different, for its transverse diameter is larger, when compared to its antero-posterior dimension, than that of any other species of *Loxolophus*.

MEASUREMENTS

	mm.
Anterior-posterior diameter	3.6
Transverse diameter	6.2

Triisodontidæ

Eoconodon, sp.

Plate IV, Fig. 5

Princeton No. 13290, left lower molar.

HORIZON AND LOCALITY.—Base of the "Puerco sandstone," south of locality No. 7 on map, text fig. 2., T56N, R99W, Sec. 1.

CHARACTERS.—This very primitive lower molar is insufficient in itself to permit a specific determination, but it compares with other specimens of the genus, and has all of the characters ascribed to the genus by Matthew and Granger, "trigonids low, metaconids and protoconids slightly connate, subequal, paraconid strong, heel large, basined." [1] The enamel on the tooth is very thin.

MEASUREMENTS

	mm.
Length	10.4
Width	5.4

[1] Matthew, W. D., and Granger, W., "New Genera of Paleocene Mammals," *Amer. Mus. Novitates*, No. 13, p. 7, 1921.

Mioclænidæ

Oxyacodon sp.

Plate IV, Figs. 2, 3

Princeton No. 13338, left lower jaw with M_{1-3}.

HORIZON AND LOCALITY.—Base of "Puerco Sandstone," southwest wall of Polecat Basin, T57N, R98W, Sec. 31, Park County, Wyoming, Locality No. 7 on map, text fig. 2.

CHARACTERS.—The form represented by this specimen probably can be separated specifically from *Oxyacodon apiculatus* Osborn and Earle and from *O. agapetillus* (Cope) when more material is obtained. On the high trigonids of the molars, the protoconids and the metaconids are subequal in size and elevation, but the paraconids are low and minute. The heels are tricusped and on M_{1-2} are wider than the trigonids and about as long as the trigonids, but on M_3 the heel is very long and is narrower than the trigonid. The high sharp hypoconulid of M_3 extends far posterior to the hypoconid and curves distinctly forward.

MEASUREMENTS

	mm.
M_{1-3}	9.9
Anterior-posterior diameters	
M_1	3.1
M_2	3.1
M_3	3.7
Transverse diameters	
M_1	2.1
M_2	2.3
M_3	1.9

B. TORREJON

MULTITUBERCULATA

Ptilodontidæ

Ptilodus sp.

Plate IV, Fig. 10

Princeton Nos. 13361, 13361a, and 13361b, lower left sectorials.

HORIZON AND LOCALITY.—"Torrejon Sandstone," S. W. Polecat Dome, locality No. 6 on map, text fig. 2.

CHARACTERS.—The drawing, Plate IV, Fig. 10, of specimen No. 13361 shows the characters which serve to place it in the genus

Ptilodus. There is an anterior basal notch on P_4 to accommodate P_3, and the flat lateral surfaces of P_4 are modified by 13 ridges which meet from either side at the crest and form a serrated cutting edge.

No. 13361b is of additional interest because it and the *Palæosinopa* jaw, No. 13322, were the two specimens first discovered in the Bighorn Basin Torrejon horizon.

MEASUREMENTS

	mm.
Length	7.2
Width	2.4

INSECTIVORA

Pantolestidæ

Palæosinopa sp.

Plate V, Figs. 7, 8

Princeton No. 13322, left lower jaw with M_{1-3}.

HORIZON AND LOCALITY.—"Torrejon Sandstone," Paleocene, southwest Polecat Dome. Locality No. 6 on map, text fig. 2.

CHARACTERS.—This specimen has the typical creodont-like teeth of *Palæosinopa* and has, in addition, the peculiarity of a mental foramen under M_1. The molars are so similar to those of *Palæosinopa didelphoides* that the great difference in size is the chief distinction between the two specimens. On M_3 the hypoconulid is slightly more prominent than it is in *P. didelphoides*.

MEASUREMENTS

	mm.
M_{1-3}	9.5

CREODONTA

Oxyclænidæ

Chriacus sp.

Plate IV, Fig. 4

Princeton No. 13288, left upper molar (M^2?).

HORIZON AND LOCALITY.—"Torrejon Sandstone," S. W. Polecat Dome, locality No. 6 on map, text fig. 2.

CHARACTERS.—The tooth differs specifically from all described species of *Chriacus*, but is not deemed sufficient to found a new

species. In outline it is subquadrangular. The sub-crescentic protocone is large, and the paracone and the metacone are low conical cusps, subequal in size. A small paraconule and a metaconule are present, and the complete basal cingulum supports a small parastyle, a small metastyle, and a prominent hypocone.

MEASUREMENTS

	mm.
Greatest anterior-posterior diameter	7.5
Transverse diameter	9.7

?PRIMATES

?Plesiadapidæ

Plesiolestes problematicus, new genus and species.

Plate IV, Figs. 6, 7

HOLOTYPE.—Princeton No. 13291, right lower jaw with P_3 and M_{1-3}.

PARATYPE.—Princeton No. 13292, right lower jaw with P_3–M_2.

HORIZON AND LOCALITY.—"Torrejon Sandstone," southwest Polecat Dome, Locality No. 6 on map, text fig. 2.

CHARACTERS.—The holotype preserves, in front of P_3, three alveoli, the anterior one of which is very large and descends to a closed end under the anterior root of P_3. Somewhat smaller, the second alveolus is also inclined, but the much smaller third one is almost vertical, parallel to the two roots of P_3. This latter tooth has a simple, high, main cusp and a heel whose posterior border is connected with the main cusp by a low ridge. P_4 is molariform. The trigonid is longer than it is wide, with the protoconid and the metaconid large, subequal, and the paraconid much smaller. The heel is wide and basined, with a pointed hypoconid, a weakly bicusped hypoconulid and an entoconid at the postero-lingual corner. An external cingulum is at the base of the protoconid, and this latter cusp has a pronounced keel or carination on its postero-labial slope.

M_1 has the protoconid and the metaconid subequal, the former subconical and the latter elongate antero-labially and postero-lingually, and a small ridge-like paraconid which gives the trigonid the characteristic quadrate shape of Plesiadapids. A ridge descends the postero-labial slope of the metaconid. The heel is wider than the trigonid, the hypoconid is large, the hypoconulid is bicusped, and upon the ridge from the hypoconid to the posterior base of the

protoconid there is a distinct cuspule. M_2 is similar to M_1, and has a somewhat wider, shorter trigonid and a wider heel. M_3 is very suggestive of Plesiadapid relationships, with the trigonid very similar to that of M_2 and a great enlargement of the bicusped hypoconulid into a third lobe.

This form is hesitantly referred to the Plesiadapidæ upon the bases of the enlarged procumbent front tooth, the shape of the molar trigonids, and the character of the heel of M_3. There are many structures on the two specimens which are not like those of other Plesiadapids.

MEASUREMENTS

	mm.
P_3–M_3	10.8
M_{1-3}	7.9

C. "PRINCETON QUARRY"—TIFFANY-BEAR CREEK FORMS

AMBLYPODA

Coryphodontidæ

Titanoides gidleyi, new species.[1]

Plate VI, Figs. 1–7

HOLOTYPE.—Princeton No. 13235, parts of lower jaws of one individual, with all teeth represented from one side or the other, except I_1, I_3 and P_1.

HORIZON AND LOCALITY.—Middle Paleocene, approximately same level as Princeton Quarry, but somewhat lower, T56N, R99W, Sec. 11, Locality No. 5 on map, text fig. 2. Park County, Wyoming.

CHARACTERS.—The I, possibly the second, has nothing except its smaller size to distinguish it from several *Coryphodon* incisors in the

[1] Species dedicated to Mr. J. W. Gidley.

PLATE V

FIG. 1.—*Litolestes ignotus*. Holotype, new genus and species, Princeton No. 13352, right lower P_4–M_3, crown view, × 6.

FIG. 2.—The same specimen, internal view, × 6.

FIG. 3.—*Plesiadapis fodinatus*. Allotype, new species, Princeton No. 13306, upper left P^3–M^1, crown view, × 4.

FIG. 4.—*Plesiadapis fodinatus*. Allotype, right upper M^{2-3}, crown view, × 4.

FIG. 5.—*Plesiadapis fodinatus*. Holotype, Princeton No. 13278, right lower dentition, crown view, M_3 supplied from Paratype, Princeton No. 13356, × 2.

FIG. 6.—The same specimens, external view, ×2.

FIGS. 7, 8.—*Palæosinopa* sp. Princeton No. 13322, M_{1-3} left, crown view, and lower jaw with same, external view, × 4.

PLATE V

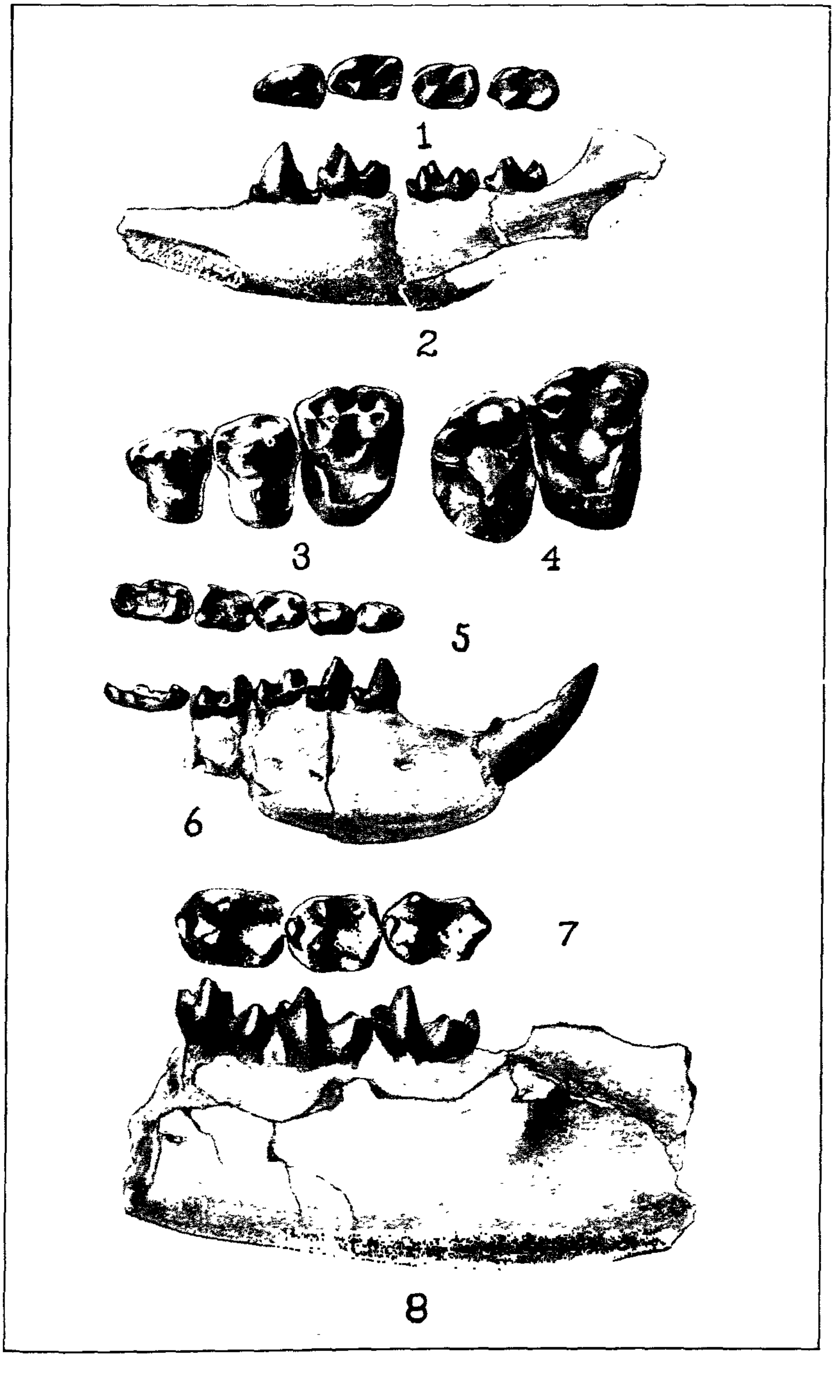
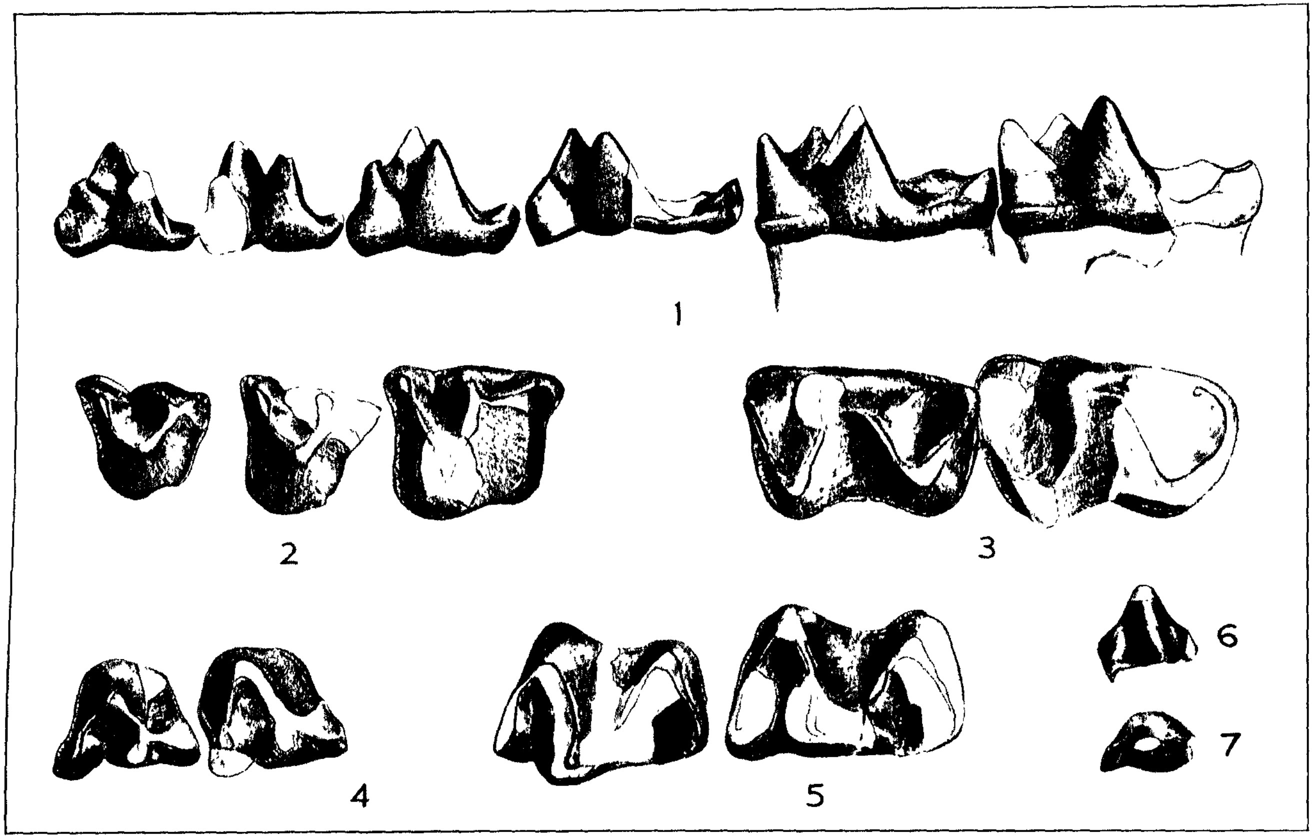


PLATE VI



Princeton collection, having a median cusp and a small accessory cusp on each side. Only a speck of enamel remains upon one canine, and the other is without a tip, but certain characters can be described from the remaining crown and root. The tooth is triangular in cross section, with the apex presumably toward the premolars, judging from a canine root tip which is preserved in a fragment of bone that does not make contact with the rest of the jaw but retains enough of the symphysial region to indicate its approximate position. What is probably the external side of the right C has an irregular surface, with a prominent bulge in its central part. The enamel extends farther down upon the rear of the root than upon the sides and front.

P_2 has a large protoconid, a small, compressed paraconid, and a small metaconid, somewhat larger than the paraconid. The complete anterior cingulum supports a cuspule at the anterolingual corner of the tooth, and the posterior cingulum, also complete, rises internally to a low posterolingual cusp. Upon premolars $_3$ and $_4$ the anterior cingula are less prominent, the posterior cingula are progressively larger, becoming a broad ledge in P_4, and the metaconids are much larger than the paraconids. P_4 has a faint but complete external cingulum.

The molars have a double V pattern of cusps and crests, with the trigonid higher, wider, and longer than the talonid. The metaconid is higher and more massive than the protoconid and the paraconid, the latter being compressed and ridgelike, whereas the two former are subconical. A strong crest connects the hypoconid tip with the base of the metaconid.

This species differs from the type of the genus, *Titanoides primævus* [1] in being considerably smaller in molar dimensions, in having the molar metaconid higher than the paraconid, a less complete external basal cingulum, and in other small details which might be either specific or individual. Probably the premolar

[1] Gidley, J. W., "Notice of a New Paleocene Mammal, a Possible Relative of the Titanotheres," *Proc. U. S. Nat. Mus.*, Vol. 52, pp. 431–435, plate 36, 1917.

PLATE VI

Titanoides gidleyi. Holotype, new species. Princeton No. 13235. All figures × 4/3.

FIG. 1.—Right lower dentition, internal view, composite drawing.
FIG. 2.—Left P_{2-4}, crown view.
FIG. 3.—Left M_{2-3}, crown view.
FIG. 4.—Right P_{2-3}, crown view.
FIG. 5.—Right M_{1-2}, crown view.
FIG. 6.—Incisor, inner side.
FIG. 7.—Incisor, crown view.

preserved in *T. primævus* is the third, and the fourth is missing from the specimen.

Titanoides seems to be a pre-*Coryphodon* stage of amblypod evolution, and may be in the direct ancestral line of *Coryphodon*, differing, however, from the latter in retaining a strong paraconid in all trigonids from P_2 to M_3 (though it is already smaller than the metaconid), and in having a strong crest from the tip of the hypoconid to the base of the metaconid instead of the weak low crest observed in *Coryphodon*. In *Titanoides* molars the connected double V pattern formed by the cusps and crests of the trigonids and talonids is complete and about equal-angled, whereas in *Coryphodon* the most anterior and most posterior crests have become much more transverse, making the V's lean toward each other.

Although originally describing it as a possible relative of the titanotheres, Mr. Gidley has, long since, regarded *Titanoides* as an amblypod.

MEASUREMENTS OF *Titanoides gidleyi*

	mm.
P_4–M_2	57.8
Anterior-posterior lengths	
P_2	13.5
P_3	15.2
P_4	18.
M_1	21.7
M_2	23.2
Transverse diameters	
P_2	11.4
P_3	11.8
P_4	15.3
M_1	15.
M_2	15.7

MULTITUBERCULATA

Ptilodontidæ

Microcosmodon conus,[1] new genus and species.

Plate VII, Figs. 3, 4, 5

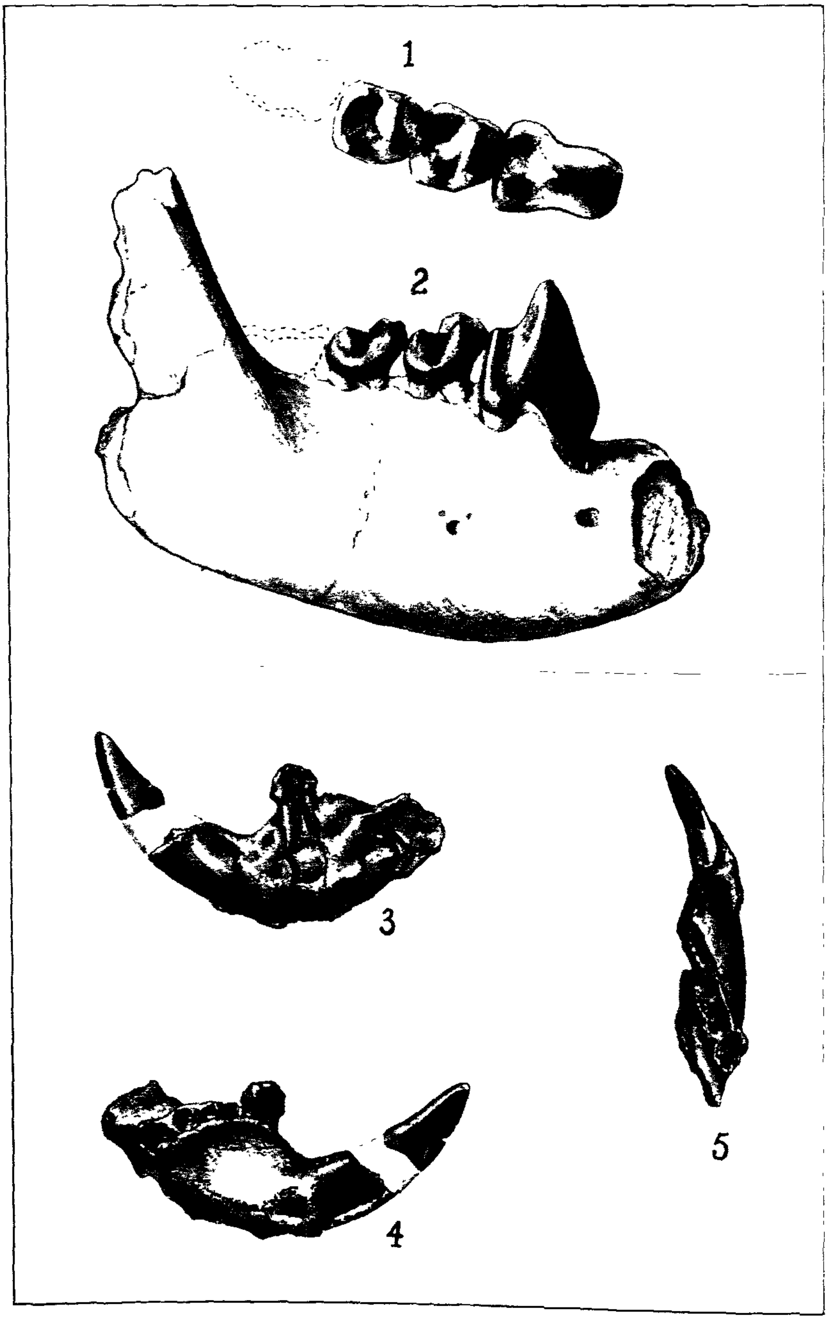
HOLOTYPE.—Princeton No. 13331, a left lower jaw with I and P_4, and alveoli of P_3, M_{1-2}.

HORIZON AND LOCALITY.—Paleocene, "Quarry Level," Big Sand Coulee, Park County, Wyoming.

CHARACTERS.—Dental formula 1·0·2·2. Large, compressed, gliriform incisor with root extending under the P–M series to an in-

[1] Named with reference to the small conical cusps of P_4.

PLATE VII



completely closed end labial to the posterior root of M_2. I cutting tip worn to a sharply curved edge. Enamel almost covers the distal end of I, except wear surface, and recedes downward away from the posterior margin, ending below P_3.

P_3 minute, single rooted, abutting P_4.

P_4 laterally compressed, smaller relative to the I than in other genera of the family. Shorter than M_1. Anterobasal border depressed to accommodate P_3. Anterior and superior borders modified by 5 cusps which are progressively larger posteriorly, the last three being subconical. Wider than the intercusp ridge, the sides of the cusps make very short anterobasally-directed ridges before merging into the widening body of the tooth. These small side ridges are somewhat similar in attitude to the many long ridges on the sides of the lower sectorial premolars of all other ptilodonts. A basal ledge projects from the posterolabial area of the tooth, making it wider posteriorly. This ledge is worn, as is the external surface below the fifth cusp, making a determination of the original cuspidation of this part of the tooth impossible.

M_1 had 5 roots, was wider and longer than P_4, and narrower than the double-rooted M_2.

The anterior border of the ascending ramus is labial to the middle of M_1.

A mental foramen opens slightly below and anterior to the anterior root of P_4.

MEASUREMENTS

	mm.
P_4 length	1.6
width	0.7
height	1.4
P–M series	5.0
M_1 length	2.2
M_2 length	1.3

Despite the wear on the I and on P_4, the conjecture that the specimen might represent a young individual, and that P_4 might be deciduous, at first seemed plausible, but examination and careful excavation under a very high-power binocular microscope revealed

PLATE VII

FIG. 1.—*Phenacolemur pagei.* Holotype, new species, Princeton No. 13286. Lower dentition, right side, crown view, × 4.7.

FIG. 2.—*Phenacolemur pagei.* Same specimen, right lower jaw, external view, × 4.6.

FIGS. 3–5.—*Microcosmodon conus.* Holotype, new genus and species, Princeton No. 13331.

FIG. 3.—Left lower jaw, external view, × 4.

FIG. 4.—The same, internal view, × 4.

FIG. 5.—The same, from above, × 4.

that the roots of P_4 are closed, the especially long anterior one extending below the superior border of the I, along its side, and that the end of the latter is almost closed.

With some reservations, the genus is at present referred to the *Ptilodontidæ* because of the dental formula, shape and enamel disposition of the I, position and size of P_3, compression of P_4 and its cuspidate edge.

The relatively small and unique P_4 serves sharply to distinguish this genus from others within the family, and may, with the future discovery of related forms, aid in a more positive classification.

Ptilodus sp.

Plate IX, Fig. 6

Princeton No. 13362, right P_4.

HORIZON AND LOCALITY.—"Princeton Quarry" level, Big Sand Coulee, Park County, Wyoming, Locality No. 3 on map, text fig. 2.

CHARACTERS.—This specimen has all of the characters of P_4 of the genus. The anterior basal notch for the accommodation of P_3 is well shown, and the lingual and labial surfaces of P_4 have 13 ridges which meet on the cutting edge and form serrations.

MEASUREMENTS

	mm.
Length	7.2
Width	2.0

INSECTIVORA

Leptictidæ

Leptacodon packi, new species.[1]

Plate VIII, Figs. 4, 5

HOLOTYPE.—Princeton No. 13296, left lower jaw with P_4–M_3 and alveoli of I_2–P_3.

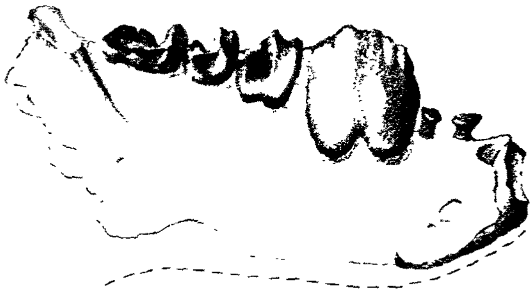
HORIZON AND LOCALITY.—Paleocene, "Princeton Quarry" level, Big Sand Coulee, Park County, Wyoming.

CHARACTERS.—I_2–M_3 in close series, no diastemata. Dental formula of lower jaw, 2(3?)·1·4·3. The incisive alveoli are in a line transverse to the C–P–M row and are both directly anterior to the

[1] Dedicated to Mr. and Mrs. Arthur Newton Pack, scholars and tutors in the exposition of natural history.

PLATE VIII

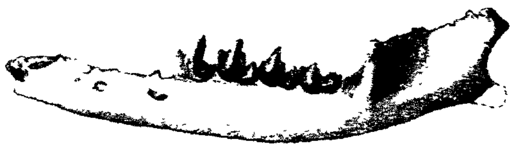
1



2

3

4



5

C alveolus. The root of the external incisor (I_3?) is smaller than that of the internal one (I_2?). C alveolus only slightly larger than that of the single-rooted P_1. P_{2-3} both double-rooted. P_4 submolariform, with high, sharp-pointed, recurved protoconid, prominent anterolingual basal cusp, metaconid lower than protoconid and posterolingual to it. Posterolabial cusp of heel helps enclose a small basin. Anterobasal cingulum less prominent, and protoconid higher, than in *L. tener*.[1]

Molars have elevated crowns and high, pointed cusps on the trigonids. Metaconids higher than the paraconids which are subconical cusps on anterolingual ridges from the protoconids and have a more lingual position than they do in *L. tener*. Heels basined, with prominent hypoconulids. In M_{1-2} trigonids and heels are about equal in transverse diameter, and the hypoconids and entoconids are subequal. On M_3 the trigonid is wider than the heel, the hypoconid is larger than the entoconid, and the hypoconulid is less isolated than it is in *L. tener*.

The jaw is very low and long. The condyle is low; masseteric fossa deep. The anterior mental foramen is under the anterior root of P_3, and the posterior foramen appears below the posterior root of P_3. The angular process projects downward and backward. Though the jaw is of about the same depth as that of *L. tener*, it is much more rounded in cross-section, and heavier.

MEASUREMENTS

	mm.
I (alveolus)–M_3	10.3
M_1–M_3	4.2

Diacodon minutus, new species.

Plate IX, Figs. 7, 8, 9

HOLOTYPE.—Princeton No. 13360, left lower jaw with M_{1-3}.

PARATYPE.—Princeton No. 13360a, left lower jaw with P_2–M_1.

ALLOTYPE.—Princeton No. 13304, fragment of maxilla with left M^{1-3}.

[1] Matthew, W. D., and Granger, W., "New Genera of Paleocene Mammals," *Amer. Mus. Novitates*, No. 13, pp. 2–3, 1921.

PLATE VIII

FIG. 1.—*Carpolestes dubius*. Holotype, new species, Princeton No. 13275. Right lower jaw, crown view, × 4.4.
FIG. 2.—*Carpolestes dubius*. External view of the same, × 4.3.
FIG. 3.—*Carpolestes dubius*. Allotype, Princeton No. 13305. Left maxilla with P^3–M^2, crown view, × 4.3.
FIG. 4.—*Leptacodon packi*. Holotype, new species, Princeton No. 13296, left lower jaw. crown view, × 4.3.
FIG. 5.—*Leptacodon packi*. External view of the same, × 4.3.

HORIZON AND LOCALITY.—All of the specimens are from the "Princeton Quarry," Paleocene, Park County, Wyoming, and were found within a few inches of each other.

CHARACTERS.—The upper molars of this tiny form appear less specialized than they are in Diacodons of subsequent horizons; but in general the molar pattern is typically Diacodont, agreeing in many respects with the Gray Bull *Diacodon*, *D. tauri-cinerei*. In this new form the paracones are larger than the metacones, the protocones are robust and low, not anteroposteriorly compressed and elevated as they are in *D. tauri-cinerei*, and on each molar, ridges run from the protocone to the paraconule and the metaconule respectively. Upon M^1 the hypoconid is much smaller than it is on M^2, and the posterior outline of M^1 is but slightly concave, quite in contrast to the great upward curve observed in *D. tauri-cinerei*. M^2 is larger than M^1. M^3 is small, triangular in shape, has minute conules and no metastyle, agreeing in these respects with M^3 of *D. tauri-cinerei*.

LOWER TEETH.—P_2 is poorly preserved, but enough remains to state that the tip is not pointed, but is, rather, compressed laterally, spatulate, and specialized for cutting. P_3 has a main cusp, more pointed than the crest of P_2, and an anterior cusp similar to that observed in *D.* (*Prodiacodon*) *puercensis* Matthew and in *D. tauri-cinerei* Jepsen. The posterior cusp is broken, but the remnant indicates its original presence. The protoconid of P_4 is larger and higher than the metaconid. The paraconid is less anterior and less isolated from the protoconid than it is on P_4 of *D. tauri-cinerei*. A sharp groove separates the entoconid from the hypoconulid, the latter being in close association with the hypoconid. Possibly wear

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PLATE IX

FIG. 1.—*Phenacodaptes sabulosus*. Holotype, new genus and species, Princeton No. 13302. Right lower jaw, permanent dentition, external view, × 4.5.

FIG. 2.—*Phenacodaptes sabulosus*. Right lower dentition, crown view, composite drawing from Princeton Nos. 13302, 13321, and 13355, × 4.5.

FIG. 3.—*Phenocadaptes sabulosus*. Princeton No. 13317. Left DP_3, DP_4, and incoming P_4, external view, × 4.5.

FIG. 4.—*Phenacodaptes sabulosus*. Princeton No. 13317. Crown view, of DP_4, × 4.5.

FIG. 5.—*Phenacodaptes sabulosus*. Princeton No. 13321. Anterior part of lower right jaw showing fully erupted crowns of P_{2-4}, external view, ×4.5.

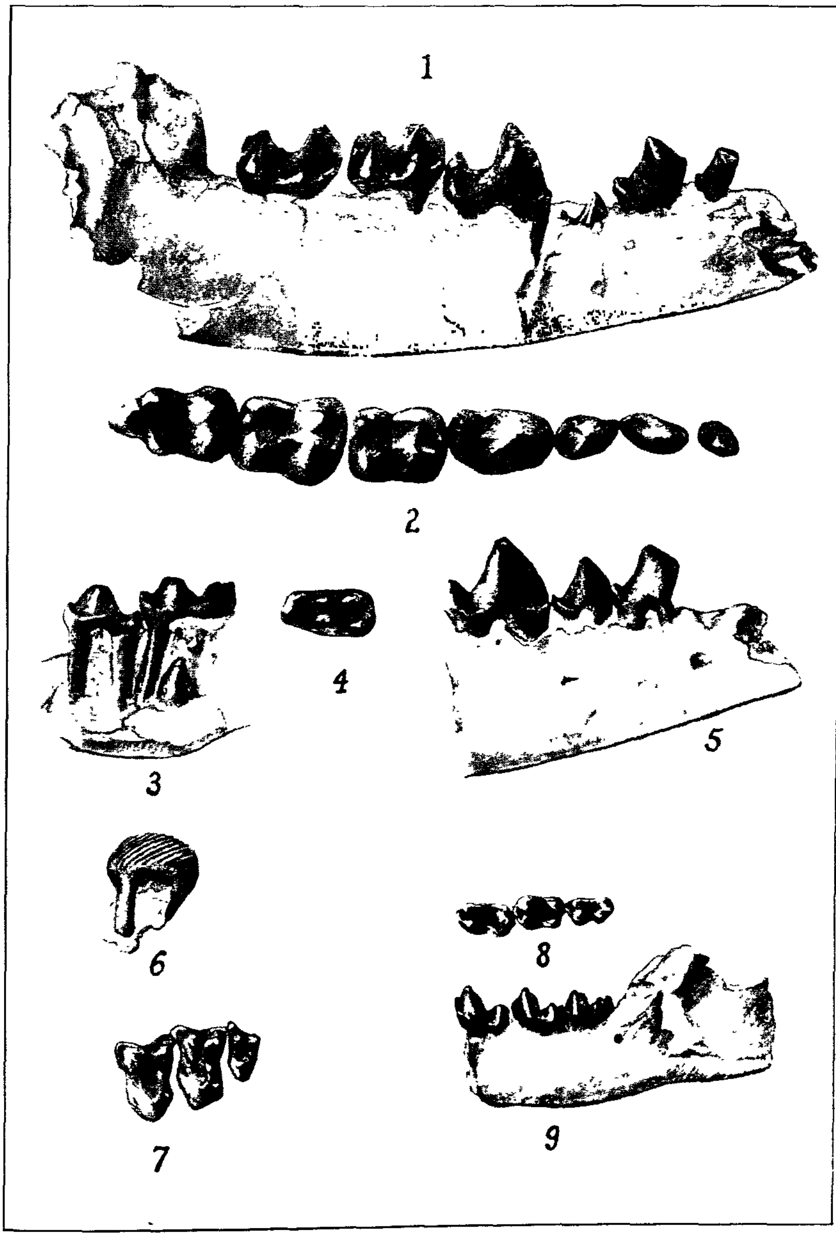
FIG. 6.—*Ptilodus* sp. Princeton No. 13362. Right lower sectorial, lingual side, × 1.8.

FIG. 7.—*Diacodon minutus*. Allotype, new species, Princeton No. 13304. Left M^{1-3}, crown view × 5.

FIG. 8.—*Diacodon minutus*. Holotype, Princeton No. 13360. Left M_{1-3}, crown view, × 4.

FIG. 9.—*Diacodon minutus*. External view of the same, × 4.

PLATE IX



is responsible for the vagueness of the crest from the hypoconid to the middle of the posterior confluency of the protoconid-metaconid.

M_{1-2} are about equal in size. Upon the trigonids, the metaconids are the sturdiest cusps and equal the protoconids in height. The paraconids are much reduced, ridgelike, and are smaller comparatively than in other Diacodons.

M_3 is smaller than M_{1-2}. The protoconid of the former almost equals the metaconid in size. The hypoconulid is very high and prominent, but not as posteriorly situated as it is in *D. tauri-cinerei*. Deeply basined, the heels of each molar have the same width as the trigonids.

MEASUREMENTS

	mm.
M^{1-3}	4.2
M^2 transverse diameter	2.5
M^3 transverse diameter	1.5

?INSECTIVORA

Litolestes ignotus, new genus and species.

Plate V, Figs. 1, 2

HOLOTYPE.—Princeton No. 13352, right lower jaw with, P_4–M_3, and an associated incisor.

PARATYPE.—Princeton No. 13348, right lower jaw with P_2, roots of P_3, and P_4–M_3.

CHARACTERS.—Dental formula ?–?–4–3. The incisor is short and flat, spatulate, similar to the incisors of *Diacodon*. P_2 is double-rooted and has a low posterior ledge. The third premolar is not preserved. P_4 is a relatively large tooth, has a high, pointed, slightly recurved protoconid and an anterior cingulum which rises to a small cusp at the antero-lingual part of the tooth. This little cusp varies in size. On the holotype it is a prominent paraconid, but on the other specimens it is less prominent. A small but distinct metaconid is present on the postero-lingual slope of the protoconid. The widest part of the tooth is at the posterior base of the protoconid, where it joins the very short wide heel. Only one tiny cuspule modifies the transverse ridge that forms the posterior part of the heel, and this cuspule is joined to the postero-labial slope of the metaconid by a short crest.

The molars diminish in size from M_1 to M_3. Each has a well developed but small paraconid, joined to the protoconid by a crest. On M_1 the protoconid and the metaconid are subequal, but on M_2 and on M_3 the metaconid is higher and larger than the protoconid.

On each molar the heel is deeply basined, the hypoconid and entoconid are large, the latter is higher than the former, and the hypoconulid is small but distinct. The hypoconulid of M_3 projects somewhat posteriorly.

The jaw is slender and is deepest under M_1. It is pierced by a foramen under the anterior root of P_2 and by one under the posterior root of P_3.

There is no certainty in the reference of this diminutive mammal to the insectivores. Its relationships to other forms are not demonstrated by the present material.

MEASUREMENTS

	mm.
P_4–M_3 (Paratype Princeton No. 13348)	6.2

?PRIMATES

Plesiadapidæ

Phenacolemur pagei, new species.[1]

Plate VII, Figs. 1, 2. Text Fig. 4

HOLOTYPE.—Princeton No. 13286, right lower jaw with P_4–M_2 and alveoli of I, M_3.

PARATYPES.—Princeton No. 13316, right lower jaw with worn P_4–M_3, and preserving the condyle and masseteric fossa.

Princeton No. 13314, left lower jaw with P_4–M_1, and showing the angle.

HORIZON AND LOCALITY.—Paleocene, "Quarry Level," Big Sand Coulee, Park County, Wyoming.

CHARACTERS.—Procumbent I root terminates below posterior root of M_1. P_4 very large and elevated, anterior margin straighter, less convex than in *P. citatus*.[2] Posterolabial margin of P_4 distinguished by a long ridge which ascends to the summit of the labial cusp of the heel. Heel broad, protoconid separated by a notch from posterolingual cusp which is more prominent and marginal than it is in *P. citatus*. Molars with higher crowns, especially the trigonids, than are seen in *P. citatus*. Paraconids slightly smaller than metaconids.

[1] Dedicated to Joseph F. Page, who helped in the discovery of this and many other specimens.

[2] Matthew, W. D., and Granger, W., "A Revision of the Lower Eocene Wasatch and Wind River Faunas," *Bull. Am. Mus. Nat. Hist.*, XXXIV, pp. 481–482, Fig. 51, 1915.

Jaw short, robust, deepest below M_1. Anterior mental foramen below the front margin of the anterior root of P_4; posterior mental foramen under the anterior root of M_1. In *P. citatus* these two foramina are more widely spaced, the anterior one appearing well in front of P_4, and the posterior one opening below the posterior root of M_1.

Angle directed downward and not inflected, as No. 13314 shows. No. 13316 exhibits the very large masseteric fossa, the plane, weakly convex, articular surface of the condyle, and the absence of a notch on the superior edge of the condylar process between the condyle and the posterior border of the ascending coronoid process.

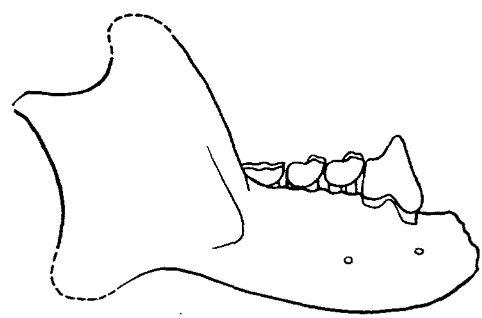


FIG. 4.—*Phenacolemur pagei.* Composite drawing from Princeton Nos. 13286, 13314, 13316, showing shape of lower jaw, about × 2.

With the aid of the paratypes on outline drawing, Text Fig. 4 has been compiled to show elements which are lacking on the holotype.

MEASUREMENTS

	mm.
P_4–M_3	11.5
P_4 length	3.3
width	2.5
M_1–M_3	8.0

?PRIMATES

Plesiadapidæ

Plesiadapis fodinatus, new species.

Plate V, Figs. 3, 4, 5, 6

HOLOTYPE.—Princeton No. 13278, right lower jaw with I–M_2.

ALLOTYPE.—Princeton No. 13306, left maxillary fragment with P^3–M^1 and roots of P^1 and P^2, and part of right maxillary with M^{2-3} and base of M^1.

PARATYPE.—Princeton No. 13356, left lower jaw with P_3, M_{1-3}.

HORIZON AND LOCALITY.—Paleocene, Princeton Quarry level, Big Sand Coulee, Park County, Wyoming.

CHARACTERS.—Lower dental formula 1·0·2·3. The incisor is long and has a raised labial side which makes a sharp cutting edge. At the posterior base of this ridge there appears a diminutive cuspule, less pronounced than the corresponding structure of *P. gidleyi* (Matthew). Between the incisor and third premolar there is a long diastema wherein the margin of the jaw is a sharp ridge (see Plate V, Fig. 6.). There is no trace of P_2. P_3 has two roots, a high pointed main cusp or protoconid, and a short, wide, bicusped heel. The rather flat posterior surface of the protoconid is modified by three vertical ridges, the middle one feebly developed. A small keel is on the anterior face of the protoconid. P_4 is considerably more robust than P_3. The anterior crest of P_4 is developed into a minute cuspule, and a small ridge appears on the antero-lingual part of the protoconid, quite in contrast to the distinct metaconid on P_4 of *P. dubius*. The tip of the protoconid has a definite outward or labial curvature, and from the point three posterior ridges descend. On the trigonid of M_1 the protoconid is the largest cusp, and the paraconid and metaconid are closely joined. The heel is very wide and the ridge from the hypoconid to the entoconid supports a very minute hypoconulid. The enamel on the lingual slope of the hypoconid and the ridge from this cusp to the metaconid are crenulate. M_2 has the paraconid and metaconid conjoined to the appearance of a twinned cusp, and a prominent ridge extending anteriorly from the paraconid. M_3 is not preserved on the holotype, but is present on the paratype, Princeton No. 13356. The tooth is worn so that the details of the trigonid cuspidation are obscure, but the heel is very long and typically Plesiadapid, with the bicusped third lobe.

UPPER DENTITION.—Short diastemata separate single-rooted P^1 from the teeth anterior and posterior to it. Between it and P^2 the bone ridge is perforated by two small foramina. P^2 is double-rooted. P^3 has three roots, a large protocone with anterior and posterior keels, a slightly larger paracone, and a small metacone, the latter closely joined to the paracone. There is a small parastyle on this tooth, a posterior cingulum and an incomplete external cingulum, but no paraconule, though there is an antero-lingual keel on the paracone. P^4 is larger and has a more quadrangular outline. The metacone is as large as the paracone, the posterior slope of the protocone has a postero-labially directed ridge, and there is a small ridgelike paraconule. An incomplete anterior cingulum supports a small parastyle.

M^1 is badly worn, but the protocone must have been very large and the paracone and the metacone are small. There is a little paraconule, and a very small, rather obscure, metastyle. The anterior cingulum is enlarged in the hypocone position, a parastyle is present, and a small metastyle appears upon the incomplete external cingulum.

M^2 is similar to M^1, somewhat larger, with more prominent parastyle and mesostyle.

M^3 is almost oval in outline, the protocone has a groove worn upon its antero-labial slope, the metacone is minute, and the metaconule is absent. The enamel at the internal base of the protocone is crenulate.

MEASUREMENTS

	mm.
P^1 alveolus–P^4	10.7
M^{1-3}	10.1
Transverse diameters	
P^3	3.2
P^4	4.1
M^1	4.7
M^2	5.4
M^3	5.2
P_3–M_2	12.9
M_{1-2}	7.4
Anterior-posterior diameters	
M_1	3.7
M_2	3.7
M_3	5.5
Transverse diameters	
M_1	3.0
M_2	3.3
M_3	3.2

?ARTIODACTYLA

Phenacodaptes sabulosus,[1] new genus and species.

Plate IX, Figs. 1–5

HOLOTYPE.—Princeton No. 13302, right lower jaw with P_1–M_2.

PARATYPES.—Princeton No. 13355, left lower jaw with P_4–M_3, and No. 13317, right lower jaw of young individual with DP_{3-4}, M_1 and unerupted P_{2-4}.

[1] Φεηακος, a deceiver, δάπτης, eater. So named because of doubt about the food habits as indicated by the dentition. *Sabulosus*, the specific name, refers to the sandy matrix of the quarry.

Horizon and Locality.—All of the seven lower jaws which are referred to this species came from the "Princeton Quarry," upper Paleocene, Park County, Wyoming.

Characters.—P_1 is single-rooted, laterally compressed, not pointed, but has a short cutting edge and a small posterior basal cusp. Short diastemata separate it from the teeth anterior and posterior to it. The main cusp of the double-rooted P_2, is recurved, supports a minute anterior cusp on a level halfway between its base and tip, and has a delicate ridge running halfway up its posterior surface. A single sharp cusp appears on the posterior border of the narrow heel.

P_3 simulates P_2 in most respects, including size, but has the anterior cusp placed at the anterolingual base of the main cusp instead of perched upon its anterior slope, has a wider, flatter heel, a greater transverse diameter, and possesses two very slender little ridges upon the posterior hemicone of the main cusp.

P_4 is much larger than P_3 and has an anterior basal cingulum which is enlarged to a ledge at the anterolingual corner, and gives rise to a small sharp ridge which ascends halfway up the front of the large sharp-pointed main cusp. The posterior surface of this cusp, when viewed from the side, is concave, and it is corrugated by three ridges. The middle ridge crosses the depression between the main cusp and the prominent posterior basal cusp, which itself has a sharp transverse crest whose ends curve anteriorly, the lingual one entering the base of the main cusp and the labial one forming a short external basal cingulum.

Upon M_1 the trigonid is slightly wider and higher than the talonid. The protoconid and the metaconid are subequal, almost opposite each other, and confluent at their posterior bases into a plane surface sharply declining into the basin of the heel. A ridge extends anteriorly from the protoconid, and then bends sharply to the small paraconid which is just internal to an antero-posterior midline through the tooth. The basal posterolingual part of the metaconid is modified by a vertical plication. The hypoconid is shorter and slightly less stout than the protoconid, to the lingual part of whose posterior face it extends a ridge, by virtue of which the heel appears deeply basined. The entoconid is smaller than the hypoconid and has an antero-lingual plication similar to the posterior one of the metaconid, but the two do not meet, being separated by a deep precipitous valley. The hypoconulid is distinct but it is joined to the entoconid by a short rib, and extends labially downward, posterior to the hypoconid, just as it does in *Diacodexis*. External cingula between the protoconid and the

hypoconid, and anterior cingula, are seen upon some of the specimens, but seem variable in prominence and extent.

From M_1, M_2 differs only by being larger, in having the transverse diameter of the trigonid greater relative to that of the talonid, and showing no paraconid, though the ridge from the protoconid is transverse.

M_3 is longer and narrower than M_2. Like the latter, it has no paraconid, but its metaconid is larger than the protoconid. The hypoconulid projects far to the rear of the subequal and opposite hypoconid and entoconid, much as it does in *Diacodexis*. An incomplete cingulum appears at the external base of the protoconid.

The third milk premolar is narrower than its permanent successor, and has a slightly more prominent basal cusp.

Quite in contrast to the permanent P_4, however, the deciduous fourth premolar is molariform, with three large cusps in the trigonid which, as a whole, is higher, longer, and narrower than the tricuspidate heel. The protoconid and the metaconid are subequal, and the paraconid, situate in the midline of the tooth, is somewhat smaller than either of these and has two lateral wings or carinations, the labial one of which meets an anterior keel from the protoconid at an obtuse angle. The heel is deeply basined and the hypoconid, about as stout as the protoconid, is opposite the smaller entoconid. The hypoconulid, though worn upon both of the specimens preserving DP_4, is subequal in size to the entoconid and appears almost entirely posterior to the hypoconid and the entoconid, and is closer to the latter than to the former. Its labial extension descends sharply as a narrowing ledge posterior to the hypoconid. In specimen No. 13355 (Plate IX, Fig. 3), the main cusp of P_4 appears between the two roots of DP_4.

The jaw is slender and slowly deepens posteriorly. Foramina pierce it beneath P_1 and below the anterior margin of P_4.

Little can be said regarding the relationships of this form until more material is collected. However, it is tentatively referred to the primitive artiodactyls upon the basis of its striking resemblance to certain of the genera such as *Diacodexis* and *Bunophorus*, which occur at a higher level in the same basin.

The seven jaw fragments referred to this species are all of young individuals, and consequently the cuspidation of the teeth is well preserved. In three of the specimens, Nos. 13302, 13379, and 13355, the permanent teeth are just coming in, and the order of replacement of the deciduous premolars and the appearance of the molars seems to be as follows, P_1, M_1, P_2, M_2, P_4, M_3, P_3. Certainly the third permanent premolar is the last tooth to erupt (see Plate IX, Fig. 1).

MEASUREMENTS

	mm.
P_{1-4}	8.6
M_{1-3}	9.7
Anterior-posterior diameters	
P_4	3.4
M_1	2.8
M_2	3.1
M_3	3.5
Transverse diameters	
P_4	2.0
M_1	2.3
M_2	2.8
M_3	2.3

INCERTAE SEDIS

Carpolestes[1] *dubius*, new species.

Plate VIII, Figs. 1, 2, 3

HOLOTYPE.—Princeton No. 13275, right lower jaw with P_2–M_3, root of ?I, and alveolus of P_1.

PARATYPE.—Princeton No. 13284, front of left lower jaw with P_4 and roots of ?I–P_{1-2}.

ALLOTYPE.—Princeton No. 13305, part of left palatine and maxilla with P^3–M^2, alveoli of M^3, and posterior border of P^2 alveolus.

Although there is no certainty that the specimen designated as allotype in this paper is cospecific with the holotype, the suggestion that they pertain to the same form is advanced upon the basis of (1) the size and occlusion of the teeth; (2) similar specialization of upper and lower molars; (3) the unique premolars, the lower premolars being similar to, but not identical with, those of other forms, the uppers unlike those of any other mammal; (4) their discovery in a small excavation within a few inches of each other; and (5) the absence of anything else yet discovered in the region similar to either.

HORIZON AND LOCALITY.—Paleocene, "Quarry Level," Big Sand Coulee, Park County, Wyoming. Also upper Clark Fork.

CHARACTERS.—LOWER DENTITION.—Formula ?I_1–C_0–P_4–M_3. Front tooth, possibly the incisor, has in section a laterally compressed oval outline. Its procumbent root extends back under P_{1-2-3} to a rounded, closed end, below and slightly internal to the anterior root

[1] Synonymous with *Litotherium*. Simpson, G. G., "Third Contribution to the Fort Union Fauna at Bear Creek, Montana," *Amer. Mus. Novitates*, No. 345, p. 9, 1929.

of P_4. P_1 lacks a crown in the specimens here described. Its single root is external to the median line of the premolar row and descends close against the external side of the ?I. The crown of P_2 is minute and of an asymmetrical knob-shape, elongated anteriorly. P_3 is higher, smaller, and with a more rounded crown than P_2. Both are single-rooted. The high and compressed P_4 differs in the following respects from P_4 of *C. nigridens* Simpson:[1] it presents a less rugose anterior edge and has less distinct vertical ridges; the posterior edge of the tooth is concave by virtue of the fact that the last ridge (the eighth?) sweeps upward into a posteriorly directed cusp; the anterolingual convex surface of the tooth curves posteriorly into the concave posterolingual half, making a compressed S-shaped internal outline above the bilobed base; there is no internal cingulum.

M_1 has two vertical ridges on the external side of the compressed trenchant trigonid and three smaller serrations on the inner side. The trigonid is higher than that of *C. nigridens* Simpson, and the enamel portion is deeper on the internal side. M_{1-2} have small but distinct hypoconulids, internal to a central line through the teeth. M_3 is trilobate, the middle lobe being slightly wider than the front lobe and much larger than the rear one. The front lobe has but two cusps; the protoconid, and a higher, directly opposed cusp. From a comparison with M_2 it appears that the paraconid and metaconid of M_3 are but a single cusp, for though in M_2 the crests which diverge from the protoconid connect with the tips of the paraconid and metaconid respectively, in M_3 these two crests, after separating internal to the protoconid, again converge at the summit of the large internal cusp, giving an irregular bow-shaped outline to the trigonid. Situate midway between, and slightly external to the protoconid and the external cusp of the third lobe, is the marginal hypoconid, the largest cusp on the tooth. The entoconid is smaller than the hypoconid, slightly posterior, and marginal. The third lobe has a large well developed external cusp which protrudes farther backward than the internal cusp. The latter is bilobulate.

The close spacing of the teeth, there being no diastemata, serves to give the jaw a very short stout aspect. There is a large foramen below P_2 and a small one beneath P_3.

Upper teeth. P^2 probably two-rooted. P^3 three-rooted. Three longitudinal rows of cusps distinguish this tooth. The external, crest-connected, shearing row has five cusps, the anterior two partly separated from the posterior three by a notch. The middle cusp of this row is the largest. A short, rounded, anteroexternal spur is formed by the two anterior cusps, though this forward elongation is

[1] Simpson, G. G., "A New Mammalian Fauna from the Fort Union of Southern Montana," *Amer. Mus. Novitates*, No. 297, p. 7, 1928.

not as great as is observed in the type of *Litotherium complicatum* Simpson.[1] The medial row consists of two cusps and a rugose crest extending from the posterior of the two to the posteroexternal corner of the tooth. Two subequal cusps, the posterior one weakly bilobed, compose the inner row. The basin between the median and the internal rows deepens posteriorly and then rises at the back margin of the tooth to a low ridge which joins the posterior cusp of the internal row and the posteriorly directed crest of the medial row. Near this crest, upon the marginal ridge, appears a small cusp between the medial and inner rows. The enamel of the tooth is wrinkled.

The pyriform P^4 has an external row of five cusps, the first two crest-connected and joined, the third and fourth likewise grouped together, and the fifth, the largest, with a slight posterior enlargement. Corresponding to the median cusp row of P^3, this tooth exhibits an irregular ridge with a cuspidate enlargement in the middle, hugging the base of the external row of cusps. The anterior and posterior portions of this ridge swing externally to the anteroexternal and posteoexternal borders, respectively, of the tooth. Situate slightly palatal to this ridge, and similar to the anterior and posterior sections of it, are short arcuate corrugations, one front and one rear, that end abruptly just short of positions opposite the second and fourth cusps, respectively, of the external row. Three cusps, the second one largest and most medial, all being inward from the tooth border, make up the internal row. A low rise connects the middle cusp to the cuspidate crest of the median ridge, dividing the space between into two basins, each of which assumes an irregular reniform shape due to the already noted arcuate corrugations. As in P^3 there is a posterior border ridge, and also an anterior one. Every ridge and cusp on this tooth presents a long, gently sloping lingual side and a shorter, steeper labial slope, giving to the whole the aspect of a succession of diminutive cuestas. The enamel is crenulated.

M^1 is of a very generalized type. There is a large protocone, and the paracone and metacone are well developed. The points of the paraconule and the metaconule are midway from the protocone to the paracone and the metacone, respectively, and lie upon the oblique ridges diverging from the protocone to the external corners of the tooth. An anterior cingulum gives rise to a small ectocone, in line with which and the protocone there is a strong hypocone, giving the tooth a subquadrate form. The external cingulum widens and bears incipient developments of parastyle, mesostyle, and metastyle.

[1] Simpson, G. G., "Third Contribution to the Fort Union Fauna at Bear Creek, Montana," *Amer. Mus. Novitates*, No. 345, March 18, 1929.

M^2 is similar but it has a less distinct ectocone, better developed mesostyle, and shows an exceedingly small hypostyle.

Enough of the skull is preserved to describe a few characters. Above P^3 opens the large infraorbital foramen. The malar process of the maxilla issues from a small area most of which is directly above M^2. Hence M^3 is socketed entirely in the gently rounded but prominent posterior tuberosity of the maxilla. The moderately arched palate is oddly perforated. The anterior wall of a large foramen is at a place opposite the middle of P^3. This foramen is possibly confluent with a foramen whose back margin aligns with the anterior edge of M^1, or there may have been a bridge between these two limits, dividing the opening into two foramina. The suture between the maxilla and the palatal division of the palatine zigzags forward close to the internal alveoli of M^3 and M^2 and then takes an oblique anterointernal trend.

An apparent diastema between P^3 and P^4 is due to fracture of the specimen and dislocation of adjoining parts.

Measurements

	mm.
Length	
P_1–M_3	11.4
P_1–P_4	6.2
M_1–M_3	5.2
P_4	3.4
M_3	2.1
P^3–M^2	7.9
P^3	2.6
P^4	2.1
Width	
P_4	2.2
M_1	1.8
M_2	1.6
M_3	1.4
P^3	2.4
P^4	2.3
External height of P_4	3.8

Postulates as to the family or ordinal position of this curious form, on the basis of present material, seem futile. It is possible to select suites of characters which, taken by themselves, would place *Carpolestes* in any one of several orders.

Its only striking affiliation is with *Carpodaptes* of the Tiffany Paleocene of Southwestern Colorado.[1] This relationship has made it seem desirable to retain the name *Carpolestes* for this form, instead

[1] Matthew, W. D., and Granger W., "New Genera of Paleocene Mammals," *Amer. Mus. Novitates*, No. 13, p. 6, Sept. 6, 1921.

of *Litotherium*, inasmuch as the former simulates *Carpodaptes*, and has precedence. If, as seems possible, *Litotherium complicatum* Simpson, A. M. N. H. No. 22196, is cospecific with *Carpolestes nigridens* Simpson, then this Bear Creek species differs from the species herein described in having much longer third upper premolars and fourth lower premolars, a difference which is probably a specific function of the aberrant specialization of these types.

D. "Clark Fork"

CREODONTA

Oxyænidæ

Dipsalodon matthewi,[1] new genus and species.

Plate X, Figs. 8, 9

HOLOTYPE.—Princeton No. 13152, lower jaws.

HORIZON AND LOCALITY.—Upper part of the "Clark Fork beds," at locality No. 2 on map, text fig. 2.

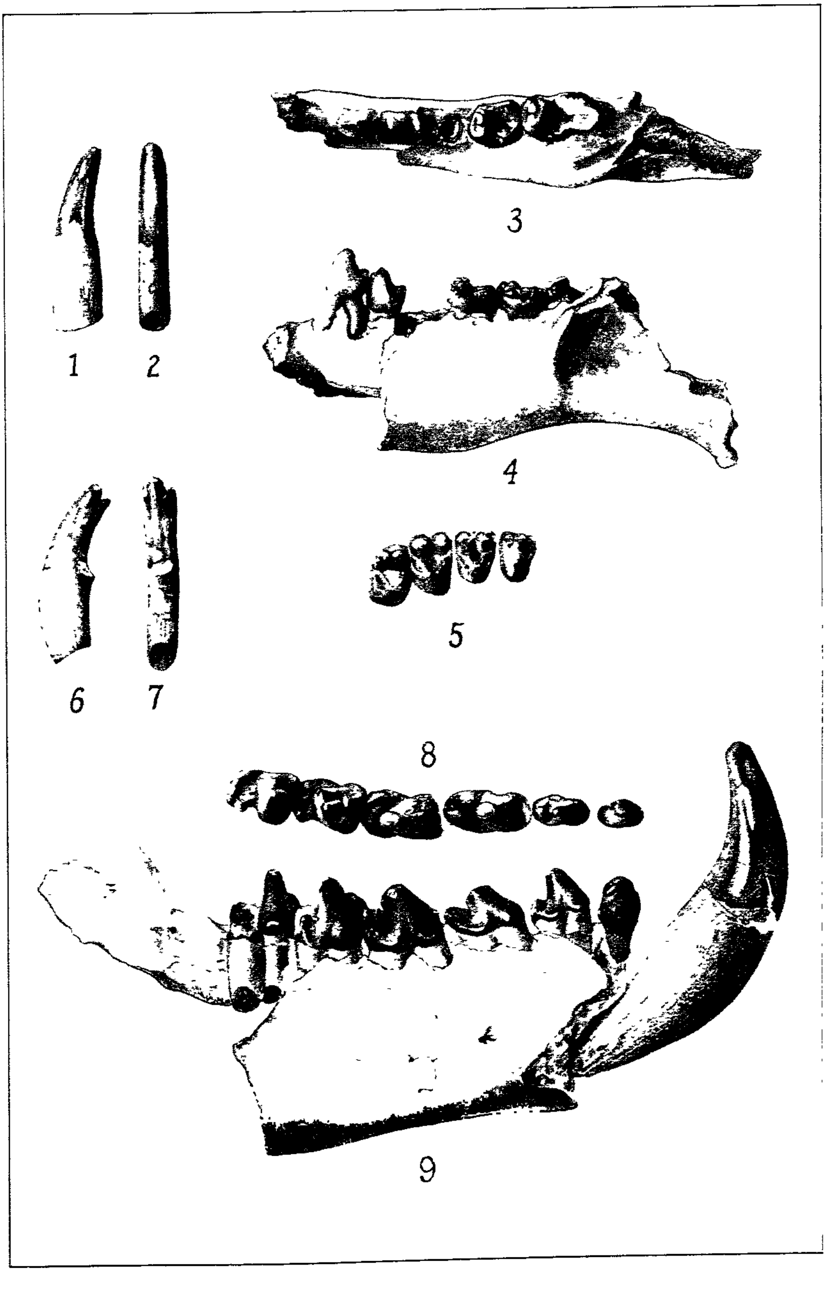
CHARACTERS.—Lower dental formula, ?–1–4–2. The canine has a long, massive root, and a relatively small slender crown. P_1 is single-rooted, simple-crowned. All of the other teeth in the P–M series have two roots. P_2, a simple tooth, has upon the main cusp an antero-lingual ridge, and carries a small posterior basal cusp. The main cusp leans only slightly posteriorly, quite in contrast to the strong backward slant of P_{3-4}. On P_3 the chief cusp is low and stout and supports a conjoined cuspule upon its antero-lingual curvature. The anterior cingulum is very faint. The heel is large and has three well defined cusps; a large external one which

[1] Species dedicated to Dr. W. D. Matthew, who, regretably, deceased while this paper was in press.

PLATE X

FIGS. 1–7.—*Plesiadapis cookei*. Holotype, new species, Princeton No. 13293.
FIG. 1.—Right lower incisor, inner side, × 1.
FIG. 2.—The same, posterior surface, × 1.
FIG. 3.—Left lower jaw, crown view, × 1.
FIG. 4.—The same, external view, × 1.
FIG. 5.—Right P^4–M^3, crown view, about × 1.
FIG. 6.—Left upper incisor, inner side, × 1.
FIG. 7.—The same, posterior surface, × 1.
FIG. 8.—*Dipsalodon matthewi*. Holotype, new genus and species, Princeton No. 13152. Lower dentition, right side, crown view, × 2/3.
FIG. 9.—*Dipsalodon matthewi*. Holotype, Princeton No. 13152. Right lower jaw, external view, × 2/3.

PLATE X



connects with the main cusp of the tooth by a ridge, a smaller internal one, and a third minor cuspule on the postero-lingual part of the tooth. A crown view (Pl. X, Fig. 8) shows that the tooth is strongly bilobed, as in P_4, and, in each case, the constriction is in about the middle of the main cusp, instead of between this cusp and the heel. The lingual cusp of the heel is separated from the main cusp of the tooth by a deep valley.

P_3 is smaller than P_4. The latter tooth has a well defined prominent anterolingual basal cusp. The main cusp is conical and is separated from the trenchant heel by a sharp notch. On the heel, in addition to the cutting cusp, which is close to the midline of the tooth, there is an elongated marginal lingual cusp.

The two molars are subequal in size. On M_1 the trigonid cusps are all well developed. The metaconid is very broad at its base and is almost as high as the protoconid, though the latter is the heavier of the two cusps. In *Oxyæna*, on the contrary, the protoconid is much higher than the other cusps of the trigonid, and the paraconid is well separated from the protoconid and the metaconid. On this new form, the three cusps of the trigonid are more closely grouped and conjoined. The heel is basin shaped, but is not as narrow or as low, compared to the trigonid, as it is on *Oxyæna*. M_2 is similar to M_1, but the heel is narrower in proportion to the trigonid on M_2.

This form is a relatively unspecialized oxyænid, with jaw very heavy and stout.

MEASUREMENTS

	mm.
P_1–M_2	80
P_1–P_4	53
M_{1-2}	27

?PRIMATES

Plesiadapidæ

Plesiadapis cookei,[1] new genus and species.

Plate X, Figs. 1–7

HOLOTYPE.—Princeton No. 13293, lower jaws and right upper I, P^4–M^3 of one individual.

PARATYPE.—Princeton No. 13342, unworn right and left M^{1-3}.

HORIZON AND LOCALITY.—Holotype from upper, or Clark Fork, Paleocene, Little Sand Coulee basin, R101W, T57N, probably

[1] The species is dedicated to James W. Cooke, '30, who discovered the first and finest specimen.

Sec. 32, Park County; paratype from lower, or Gray Bull, Eocene, between Paint and Pat O'Hara creeks, Park County, Wyoming.

Characters.—Dental formula $\frac{1\ 0\ ?\ 3}{1\ 0\ 2\ 3}$. The long curved upper incisor, I^2?, (Plate X, Figs. 6, 7) divides at its tip into three "prongs"; the middle one is large and straight, the slightly smaller labial one deflects outward, and the very small inner one, though almost completely worn away, must have deflected at an angle from the middle cusp. The only evidence that a posterior basal cusp may have been present is a worn area and a ledge. Midway down the enamel the anterior surface is rounded, but the posterior face is plane and modified by four ridges and three interspaced grooves. The two marginal plications are sharp-edged, and the middle ones are gently curved.

P^4 has a large almost conical protocone, a smaller paracone directly opposite, but separated by a deep notch (Pl. X, Fig. 5), and a minute metacone upon the posterior slope of the paracone. The incomplete anterior cingulum supports a little parastyle, but the posterior cingulum is without styles, and there is no metastyle. There is no trace of a paraconule.

The protocone of the subquadrate but tritubercular M^1 is larger than the metacone and the paracone, and has three ridges upon its internal surface; one each to the paraconule and the very small metaconule, and one, visible on unworn teeth, between the other two. A basal cingulum almost encircles the tooth, but is broken into a series of irregular wrinkles radiating down the lingual slope of the protocone. From the posterolingual part of the cingulum rises a small hypocone which is very closely joined to the protocone. A small parastyle, a mesostyle, and a metastyle also modify the cingulum, and are connected with the paracone and the metacone by tiny ridges.

M^2 is larger than M^1; but the two teeth are similarly constructed, differing only slightly in proportions. M^3, however, is comparatively flat and has a number of unique characters, the most evident of which is its suboval outline. The posterolingual part of the tooth is extended, but the hypocone is only a slight warp running from the cingulum part way up the long posterior slope of the protocone. All of the cusps and styles, save the protocone, paracone, and metacone, are reduced, the metaconule being barely evident. Upon all of the molars, but particularly upon the third, the enamel is plicated.

Lower teeth.—The lower incisor is simple, elongate, and has a rounded, somewhat flattened, tip. Raised edges form a marginal

groove on each side of the convex central warp on the posterior surface, Plate X, Figs. 1, 2. There is no basal prong. A long diastema separates the incisor from P_3. The latter tooth has two roots, and a high crown with an elevated main cusp, the protoconid, and a wide bicusped heel. Neither a metaconid nor a paraconid is developed, but small sharp ridges appear in the appropriate positions for each. P_4 is stouter than P_3 and has a diminutive paraconid and a ridge similar to that of P_3 in the metaconid position. An anterior cingulum is very weakly present. M_1 is not preserved on the type. M_2 has a subquadrate-shaped trigonid, due to the ridges which run anteriorly from the protoconid and paraconid and then bend toward each other and meet in the middle of the anterior margin of the tooth. The metaconid is situate posterolingual to the paraconid and connects by a small ridge to the protoconid. The heel is low, slightly wider than the trigonid, the hypoconid is a robust cusp, and the entoconid is ridgelike. An external cingulum is interrupted at the base of the hypoconid.

M_3 has a trigonid much like that of M_2, but the heel of the former is very long and low, the multicusped hypoconulid forming a lobe as long, but not as wide, as the hypoconid-entoconid segment. A cingulum appears at the base of the hypoconid.

All of the P–M teeth have crenulated enamel. The jaw is very stout and heavy, and the angle projects downward as far as it is preserved. A large foramen penetrates the external surface of the jaw, just below the anterior root of P_3.

Although this species is closely related to *Plesiadapis tricuspidens* from the Thanetian and to *P. gidleyi* of the Tiffany, it also shares a number of characters which Simpson has used as diagnostic of *Platychærops*,[1] the type of which is *P. richardsoni* from the London clay (Eocene) of Herne Bay. Like the latter, the species herein described has no paraconule (sometimes called protoconule) on P^4, and the molar mesostyles are strong; but in the other characters which distinguish the *Platychærops* from the *Plesiadapis* group, it agrees with *Plesiadapis*. For this reason no new genus is proposed to receive this new form, but its intermediate position may indicate that the differences between *Plesiadapis* and *Platychærops* should be regarded as subgeneric or merely specific, as Teilhard has concluded.[2] Additional material may show the variation range to be wider. A further urge toward this conception is the fact that in time range also *Plesiadapis cookei* is intermediate; for although the European

[1] Simpson, G. G., "Paleocene and Lower Eocene Mammals of Europe," *Amer. Mus. Novitates*, No. 354.

[2] Teilhard de Chardin, Pierre, "Les Mammiferes l'Eocene inferieur Francais et leurs Gisements," *Annales de Paleontologie*, 1916–21.

Plesiadapis is Thanetian and *Platychœrops* is Sparnacian (when the genera are divided upon Simpson's basis) and *Plesiadapis* has heretofore been found only in the Paleocene of North America, *P. cookei* is from both the upper Paleocene "Clark Fork" and the lowest Eocene Gray Bull.

The drawing, Plate X, Fig. 4, shows P_4 set too low in the jaw. Probably the tip of the tooth should be on a line with the top of P_3.

MEASUREMENTS OF *Plesiadapis cookei*

	mm.
M^{1-3}	17.9
P_3–M_3	30.4
Anterior-posterior diameters	
P^4	4.4
M^1	5.5
M^2	5.8
M^3	5.9
P_3	4.6
P_4	4.6
M_2	6.0
M_3	9.4
Transverse diameters	
P^4	6.6
M^1	7.0
M^2	8.6
M^3	7.6
P_3	3.6
P_4	4.2
M_2	5.5
M_3	5.5

DEPARTMENT OF GEOLOGY
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The William Berryman Scott Research Fund.

ADDRESSES MADE AT THE DINNER COMMEMORATING THE ONE HUNDRED AND FIFTIETH ANNIVERSARY OF THE GRANTING OF A CHARTER TO THE AMERICAN PHILOSOPHICAL SOCIETY HELD AT THE HOTEL PLAZA, NEW YORK CITY, WEDNESDAY EVENING, APRIL 2, 1930, THE HONORABLE JOHN W. DAVIS PRESIDING.

MR. DAVIS: Sir Robert Falconer, Mr. Farrand, distinguished guests, and "liberal and ingenious men": If there is any one here who doesn't recognize himself under the latter title, I ask him, if he is a member of the Philosophical Society, to be assured that by express law that badge of honor has been conferred upon him. I call his attention to the pamphlet which has been placed at each plate, from which he will observe that in the year 1780 the General Assembly of the State of Pennsylvania enacted a statute, two paragraphs of which thus describe the present and future members of this most distinguished organization. I read: "Whereas the cultivation of useful knowledge, and the advancement of the liberal arts and sciences in any Country, have the most direct tendency towards the improvement of agriculture, the enlargement of trade, the ease and comfort of life, the ornament of society, and the encrease and happiness of mankind;

"And whereas this country of North America" (which, of course, includes Canada, Sir Robert) "which the goodness of Providence hath given us to inherit, from the vastness of its extent, the variety of its climate, the fertility of its soil, the yet unexplored treasures of its bowels, the multitude of its rivers, lakes, bays, inlets, and other conveniences of navigation, offers to these United States one of the richest subjects of cultivation, ever presented to any people upon earth;

"And whereas the experience of ages shows that improvements of a public nature, are best carried on by societies of liberal and ingenious men," &c, &c, &c.

I am glad that wasn't written during the present debasement of the English language, or some careful soul would certainly have put it "liberal and/or ingenious men."

It is in such terms, gentlemen, that this society was baptized by the General Assembly of Pennsylvania in 1780, although its foundation, as I believe history will thoroughly establish, goes back to the year 1727.

Tonight we celebrate the one hundred and fiftieth anniversary of the granting of this charter.

Mr. Baker was kind enough to show me the other day a record of the feast held in the City of Philadelphia to celebrate the centennial anniversary of that same great event; and as I read it, I concluded that perhaps in the last fifty years we had not improved on either the vigor or endurance of our predecessors.

The menu of that dinner showed that there were three kinds of fish, the hot meats were six in number, there were three kinds of fowl served, with other things in proportion, while the choice of wines offered was ten or more. This last was of obvious design in order that there might be a new wine for each toast, for there were ten toasts as well.

The opening toast was to the Society itself, and was responded to by its President. He covered a good deal of territory. He started with Benjamin Franklin, our first President; he eulogized David Rittenhouse, the astronomer, our second President; he divagated somewhat on Thomas Jefferson, the third President; he tarried long enough to speak of the great-grandchildren of Benjamin Franklin, Bache Franklin and Dallas Franklin; he touched on the distinguished Dr. Wistar, who was their successor; and I am not sure that he didn't call the roll from the beginning down to the moment when he, himself, had taken the floor.

We shall not undertake a similar excursion tonight. I have no historical commission of that sort; but I do think we should hear a telegram from the present distinguished scientist who heads the Society:

"I regret very much that I am not well enough to be with you at dinner tonight to commemorate the one hundred and fiftieth anniversary of the granting of the charter to the American Philosophical Society.

FRANCIS X. DERCUM."

And I may couple with that the reassuring statement that I understand his health is steadily improving.

So, gentlemen, we are met tonight to celebrate this anniversary. I shall not have the privilege of presenting ten speakers to you; but I hope that what we lack in quantity, we will more than make up in quality.

I am sure you will agree that we could be no more highly honored than to have one of our neighbors from the northern border, and the head of a great institution of learning in Canada.

I think the habit of this Society is to focus its attention on some particular topic. Tonight apparently, the theme is to be education; and I have great pleasure in presenting to you Sir Robert Falconer, who will speak on "American Influence in the Higher Education of Canada."

SIR ROBERT FALCONER (University of Toronto): Mr. Davis, Guests, and Members: It would be a high honor to any Canadian, and I deem it an especial one to myself, to be asked to come to New York and speak tonight at such an important gathering as this, in celebration of the one hundred and fiftieth anniversary of one of the greatest societies for the promotion of higher learning on this continent.

I do not intend to say anything tonight in regard to the accomplishments or history of the society, but I thought it would not be out of place if I were, in a very brief way, to give you an outline of the influence which, it seems to me, has been brought to bear upon our higher education in Canada by this country, which is such an intimate neighbor to us, and which, in a very unique way, stands beside us and is bound to act upon us in our future. It is a relationship that probably cannot exist, or at least does not exist, between any two other peoples on the face of the earth.

We hear a great deal about the line, the invisible line, along which there are no soldiers or forts on either side. Well, we are accustomed to that, but Mr. Chairman, you as a lawyer, an international lawyer, know that that has not been altogether a matter of good luck, and that sometimes we came pretty close to trouble. The present condition, and I believe the assured situation in which we find ourselves, is one for which we ought to be very devoutly thankful, because troubles, economic and others, are bound to arise and I suppose as people grow in strength they will assert themselves more and more.

There does exist between us a very remarkable relationship which stands before the world as an example of what the rest of the world may arrive at by and by. If it is possible for them to reach the state of mind in which we on this continent are, the world will be a much happier place than it is today.

For that reason it is always a great pleasure for me to come to the United States, as I do frequently, and to promote in any little way, as far as I possibly can, increasing mutual understanding between our peoples. We are two sections of the English-speaking world, two powerful sections of the white race, which we were told the other day in Toronto, by Professor Gini of Rome, is now in a static condition. If we are two of the most powerful sections of the English-speaking peoples, and if the white race is becoming static—the future of the world must depend to a great extent upon our influencing one another in the right way.

Therefore I have thought that from that point of view it might be interesting to glance at the effect, as I see it, that the United States has had upon us Canadians in our higher intellectual life. To understand that, it will be necessary for me very briefly to show you something of the way in which our population came to be what it is, and how it is stretched across the Dominion.

The oldest part of the Dominion is the Maritime Provinces (I am leaving out the French section because the influence of

the United States on our French section has been, as far as I have been able to study it, very small indeed). The oldest part of English-speaking Canada came over from New England before the American Revolution—about the year 1760—a few years after the Acadians were expelled in 1755, and from that day to this in the valleys of Nova Scotia there has been probably as genuine old New England stock as you will find anywhere in the world.

If you are interested in names, you may compare those for instance on Cape Cod with those around Nova Scotia; such names as Nickerson, Crowall, and others are common in both regions. Those people were attached very strongly by ties of nature to the New England States at the time of the Rebellion; and if it had not been for the garrison at Halifax (Halifax being founded to offset Louisburg in 1748) and for the Scotch and Irish settlements that came into eastern Nova Scotia, from 1770 to 1773 the province would have gone with Massachusetts, as it then was. Maine was simply a section of Massachusetts in those days, and Nova Scotia came right up to the border of Maine; there was no New Brunswick.

Then came the Loyalists, a different type, but old American also—very old American. These Loyalists were of two classes. There was the upper class—the Tory as he was called—and there was the average Loyalist, socially of no high rank. Those Loyalists came into Nova Scotia and New Brunswick. They are the backbone of New Brunswick today. Many more entered Ontario, around the west end, by Niagara, and the east end; and they formed, in Upper Canada, the first stratum of the English-speaking Canada of today, just as they formed the second stratum of the Maritime Provinces, the first stratum being old New England.

Those Loyalists, of course, came for a purpose and with a principle. They believed in maintaining the connection with the Old Country, and the upper classes shared the social views of the old country folk. Education as then given in Oxford and Cambridge and in the schools of England, was what they desired to perpetuate in their new environment.

The Rector of Trinity Church of New York, Charles Inglis, became the bishop of Nova Scotia, the first outside of the British Isles. King's College now Columbia, (you still have the crown on your flag-pole) sent Cochrane, one of its professors, up to Nova Scotia where he became a professor in King's College at Windsor, established in 1789, the oldest university in the Empire, outside of Britain. A few years ago it was moved to Halifax.

Another King's College was established at Fredericton in 1801, also by the Loyalists, who were afraid of "Bostonianism." Some of them were graduates of Harvard. They didn't want to send their boys over to Boston to Harvard, and they besought the Government to establish an academy, and to give it a charter. That charter was given in 1801.

A third King's College was established in Toronto in 1827, although Governor Simcoe in 1798 had asked the Legislature to set aside 500,000 acres of lands—wild lands—for the maintenance of a university. Three King's Colleges were thus established to maintain a certain type of tradition for a certain class of people—the professional classes, the lawyers, and the commercial classes of the higher grade.

The first schools established in the province of Upper Canada were not common schools for the ordinary people, but grammar schools as feeders for King's College. That type was not influenced by the school system then existing on this side of the line. They took their example from Britain.

Early in the nineteenth century a large immigration arrived from the old land—Scotch, English middle-class, and Irish from the north of Ireland. The majority did not belong to the church of England, and they resented the exclusive charter of the University that required subscription by professors to the Thirty-nine Articles, and for many years there was keen political struggle over this in Canada. The educational and the political issues were interwoven.

As time went on the charters were broadened and the restrictions were removed, but in the meantime new colleges had been formed on a new basis. Dalhousie College, in

Halifax, founded about one hundred and ten years ago, by the Marquis of Dalhousie, on the model of Edinburgh; Queen's at Kingston in the same way in 1841 (Edinburgh University is mentioned in the charter); McGill in the same way in 1821.

Toronto, on the other hand, was mainly English. Cambridge, Oxford and Dublin to some extent have been the strong influence there.

But there was also a rising demand for education for common people.

They said, "We want a college education for average people"; and through them there came contact with the United States. The two colleges in Canada that have felt that contact in the past years most were Acadia College, in Nova Scotia, and Victoria College, in Ontario, which is now one of the federated colleges or universities of the University of Toronto.

Acadia College drew a great many of its people from the stock of the old pre-Loyalists, New England men. Their connection with Yale has always been very strong. Their most brilliant graduate is Jacob Gould Schurman, who after he had graduated at Acadia, went to London, to Edinburgh and Germany, and then came back to Dalhousie, and thereafter accepted a professorship in Cornell, where my distinguished friend, Dr. Farrand, succeeded him.

About fifty per cent of the graduates of Acadia College have found their life's work in the United States. No college in Canada has sent so many of its graduates over here.

Victoria College in Ontario was ministering to people not of the commercial and the official class, but more of the farming class and frontier class, and their first professors were educated either in Pennsylvania or in Wesleyan College in New England. They took back the curriculum that was so dominant in the eastern States for so long. That is a general outline of the past.

Now will you suffer with me just for a few moments while I remind you of what you know better than I do—that is, an outline of your higher education in the United States, be-

cause I want to show how that has reacted on ours. I shan't say anything about medical education until I finish with the College of the Liberal Arts.

The old College of Liberal Arts that you (so many in the United States) look upon as being distinctively American, has had its influence in Canada, as I have said. You know how long that curriculum lasted; you know how influential in the University of Pennsylvania was that cantankerous Professor William Smith, who came out from Aberdeen, and yet one of his predecessors says that the program that he laid down in the University of Pennsylvania for liberal studies became the basis of the curriculum of the American Liberal College afterwards. You know how in 1827 the Yale report spoke of the uniform standard of the curriculum of Liberal Arts, and in 1842 President Wayland said it was so uniform that a student in one New England college could go to any part of the eastern States and find himself at home as far as liberal education was concerned.

I think, Mr. Chairman, that some American writers have over-emphasized the uniqueness of the liberal college as a contribution of the American people to higher education. A great deal of its character is derivative, I think. I, myself, believe that the State University has been a far more unique American contribution to education than the College of Liberal Arts has ever been. As far as I can see, the College of Liberal Arts is very similar to the university arts faculty in Scotland, and even in parts of England; but your State University was something quite different.

You know how until probably the seventies, studies were in the doldrums in these eastern colleges. Ticknor did give a stimulus to Harvard after he came back from Germany, but it died out, and the colleges just drifted in the prescribed curricula; and then in the seventies the change came, when brilliant young Americans came back from Germany and brought new methods.

I shall say something about Dr. Welch later in dealing with medicine; but the great change was the creation through

President Gilman of the Johns Hopkins University, and through it came the next great reaction upon our higher education in Canada. Beginning about 1879, when our Professor Alexander of Toronto was the first to come down here, there has been a succession ever since of some of our ablest Canadians, most of them either retired or on the point of retiring—one of whom is our Dean James P. McMurrich, with whom many of you are acquainted.

These young Canadians came down to Johns Hopkins at the time when under Gilman, there were men like Gildersleeve and Sylvester and Roland and Martin, and others, and they felt that they had come into a new world, as it was indeed a new world, and it was a joy to live in it. They went back to Canada and carried thither a new life and a new impetus and a new hope and a new ideal, all from Johns Hopkins.

Thereafter the idea of post-graduate work developed in the United States at Harvard, Columbia, Chicago, and elsewhere, and our Canadians have come over and have discovered the meaning of graduate training, and have carried that idea home. As you got it from Germany, so it has come to us through you.

That, to my mind, is perhaps the most important contribution that was made to our higher education in Canada in the last quarter of a century.

The next comes from what I think is your greatest original contribution which began in 1837 with the birth of the Michigan idea. You had the New Englander going to the west; you had the man from the coast going out into the pioneer tract; they created new ideas, the idea that education was not meant for a select few. With the old New England idea of the community as a whole working, they took hold of education, and said, "We will have education for the people and by the people, a lower education as it were, but crowned by the higher education of a University: all for the people, supported by the people and all without hindrance from religion or race."

With that, of course, there was bound to be a change, and a very great change in the kind of studies. The old College of

Liberal Arts, just as in England and Scotland, trained gentlemen for the professions. That was not the idea of the new university. The State University has reacted on and taught many things to the other universities.

Occasionally you hear a lament about the passing of the old liberal education, but Mr. Chairman and gentlemen, the old liberal education was for a narrow circle of people in an old world. Ours isn't an old world; ours is a new world with all sorts of new conditions, and the new education has to fit the people for the new conditions; and we, north of the border, are just like you. Consequently, the State University has influenced us.

When our new country opened up in the west, our new universities became provincial universities, and they are strong and growing rapidly. That Michigan idea, as you know, was further developed in Wisconsin, which said, "We will not only educate the people, but we will see that our experts in the universities tackle the problems that are urgent in the State." They did all sorts of research, particularly farming research, for the people. Whether they went too far or not is another question, but they did undertake new university tasks.

The farming population with us, north of the line, has watched the farming population south of the line, and the result is that a similar kind of thing is going on in our universities in the west as is going on in the great State Universities of the Middle States. They also, like those south of the line, have their extension classes. They have developed that type of education more than any of the old eastern universities, although we are following in our own way. Of course the extension idea is not confined to them. It is found also in Britain, but in the west it acted with a freedom, with a scope, with a reasonableness that perhaps is meeting a very definite need that was not so obvious in other parts. Consequently the State University idea has been a powerful idea among our rising young universities.

Perhaps the most influential movement that has come to

us from the United States has been in connection with the organization of the high schools. Your high school, roughly, fills in four years, when a boy is between fourteen and eighteen. We took that over from you, and now have high schools for boys and girls between fourteen and eighteen. But a reaction against this narrow type is occurring among us as among you. Fourteen is too late to begin high school work. Differentiation should commence earlier. We have as a result been very weak in language. The differentiation should begin at twelve, at least, or eleven, and then a transfer should be permitted after that. If boys maturing later show that they have the qualities to go on further, these exceptions can be transferred to the high schools. In Canada just as in America work is being done in the colleges of Liberal Arts, two years of work, that in Europe is done by the best boys at school, and better done at school than can be done in college; you are creating junior colleges to fill in this gap.

Whether we can get away from it, I don't know, but there is no reason why, in my judgment, our people who have just as good brains as the Europeans have, should not be able to come up to the university at eighteen or nineteen as far advanced in language and in mathematics and in the sciences as they are in Europe, and then be ready for a new move. We shouldn't have to be bolstering up all sorts of preparatory classes. It may have been a necessity in the circumstances, and changes cannot be made suddenly. But we have the same problems as you have, by reason of the kind of boy or girl that we get from the schools at that age.

My time is running fast. I didn't know I had spoken so long. Now, very briefly about medicine.

I have said that the two influences, so far as I see them, that have been most powerful, are the transmission to our universities of the graduate school and the State University idea. The third influence has been in medical education.

In 1826 three-fourths of the medical practitioners of Upper Canada had been trained in the United States, of course, trained in the proprietary schools.

In 1828 McGill Medical School began. Four men came out from Edinburgh and established it, and it was the first school on this continent to put into practice the teaching methods of Edinburgh, which Edinburgh got from Leyden, and which at that time made Edinburgh the greatest medical school in the world. The reason McGill stands so high in medicine and always has stood so high is because of its origin and the unbroken tradition it has had.

It was different in Toronto. The Government, refused for many years to have anything to do with professional training, and so three proprietary schools sprang up there. Then, when about twenty-five years ago, a change was made in University organization a medical faculty was re-created in Toronto which rapidly has risen to eminence. But many of the ideas of the higher training in medicine, have come through Johns Hopkins.

I need not tell you—you know far better than I do, although I have the honor of having known him for many years now—that the greatest man, in my judgment, of leaders in medical education is Dr. Welch. When he came over in 1878 from Germany and began here in New York, he brought some entirely new ideas; and when President Gilman called him, in 1885, to Johns Hopkins, he revolutionized Johns Hopkins, and from that day to this, he has been a marvelous factor in developing that whole side of education on this continent. No one in the United States in the field of medicine is more respected by our leading men in Canada than Dr. Welch.

Dr. Welch, however, brought Dr. Osler from McGill. He was a genuine Canadian. He first took his arts in Toronto, then went to McGill for medicine, and then to England. Osler took something with him to Johns Hopkins, as Dr. Welch tells us, and that was the clinical clerkship idea that he got in London. He also brought the German clinic idea to Johns Hopkins, and so made that a marvelous clinical school. Both by reason of his origin, his close affinities with Toronto, and with Canada generally, and our pride in him, as well as the

personal charm latent in him, whatever Dr. Osler said in medicine went in Toronto and in Canada generally. In addition to all this, we owe a great deal to the United States for the marvelous generosity that has been shown to Canada by those two great foundations, the Carnegie Foundation and the Rockefeller Foundation. Whatever criticisms may be made by nations outside, about the political attitude of the United States to the world at large (and as you know, there are criticisms) I think that everyone must acknowledge that through the Rockefeller Foundation and the Carnegie Foundation, the real idealism of the United States has been manifested in a magnificent way—simply a superb way. It has been quite unique how these two foundations have gone to the ends of the earth and have carried the blessings of their immense wealth to relieve disease and discover its sources. Such a broad spirit and far-seeing policy I can't think has ever before been shown anywhere, and we in Canada have had our generous share. Through gifts from these Foundations our medical schools have been very greatly bettered and built up.

Before sitting down may I just say this: That we go on our own ways; we are bound to go on our own ways because of the social differences in the people, because of the differences in heredity, and because, in many ways, of the races out of which the people are built up. We are homogeneous people in Canada, and so far we haven't the large cities in any number that you have. We have a relatively larger rural population, and as I say, are much more homogeneous than you are. Therefore, many of the problems which I see are agitating the colleges of the United States do not affect us nearly so severely. Our students on the whole, haven't as much money as the students who are in the American colleges, and many of the problems that arise with the opportunity of spending large sums of money are mitigated.

Then, we have a more homogeneous background. The difference in that respect was borne in upon me about a year ago, when Dr. Larned of the Carnegie Foundation came to Toronto to see whether we would coöperate with him with

respect of an investigation he was holding, as to the standards in the schools and colleges of Pennsylvania. He had drawn up large lists of questions on which he was holding examinations to arrive if possible at the quality of the teaching in the different institutions; and he thought that it might be illuminating if he could get an example from Canada also, to compare with the Pennsylvania colleges, and particularly so because our honor system was so different from the American methods of education.

However, although our professors gave the most careful consideration to it, and naturally, we wanted to do anything we could to fall in with his views, it was found that the questions that were put involved such a different social background that we felt they would not be a very satisfactory basis for comparison. Questions that might be easily answered by people in the State of Pennsylvania could not be answered in the same way by boys who had been brought up in our schools in Ontario; and I have heard more than one say that the questions themselves showed that there was a really greater difference than might at first be suspected that there would be.

In regard to the social life of the students there are differences too. We have fraternities that came from the United States, but our fraternities apparently do not play quite the part that they do in the American colleges. It may be so again because of the different social background. Our athletics also are different—our football isn't the same as the American football, although it has been changed from the English football in the direction of the American game; but our problems in athletics (as you may see from the Carnegie report on athletics) are not as acute in many ways as those in your colleges.

Now I have done, Mr. Chairman. I simply want to say this: That while we go on our own way, we are so close to you, we are so similar in origin, we have so many of the same economic and social problems arising out of similar environment, that we are bound to look at life and at higher education in a much more common way, or there must be much more in

common between us in our looking at it, than in the case of any two other peoples. We owe you a great deal, as I have said, in regard to the graduate work, the State University, and medicine; and we owe you a great deal by way of the inspiration that you have given us in the superb development that has taken place in your higher education.

When we come to the States we see your universities equipped so magnificently that sometimes we almost lose heart. With great libraries and laboratories you can more adequately meet your material needs than we can. And so, although we lag behind in that way, you are an incentive to us.

And I must say also, no people could be more generous to us than you have been. You have taken our graduates in large numbers. Tonight there must be at least half a dozen Canadians present, graduates of institutions who now hold high positions in the United States. We have the names of at least six hundred Canadian graduates who occupy academic positions in the United States. The United States has received them with the greatest bounty and the finest welcome; and we can only be thankful to you for the kind spirit that you have always shown.

At the same time, the profoundest influence in our higher education has come to us from Great Britain, our fundamental ideas have been brought to us from that old land, and while they are changed as they have come to us, through our environment, they are nevertheless our most ruling conceptions in education. I have no doubt however that we Canadians are making a contribution of our own that is quite different from anything that we have received from other people; but that is another story altogether.

I can only thank you for the way you have heard me, and I must apologize to you and Dr. Farrand for having taken so much time.

MR. DAVIS: I didn't want an apology. The Chair doesn't feel under any obligation whatever to accept it. Sir Robert told me he contemplated speaking only fifteen

minutes, and I assured him this audience would not be content with any such half ration as that.

For his most interesting and inspiring speech, I am sure we are all profoundly grateful. It touches on one aspect of a very striking phenomenon. The theme might be pursued with equal interest and profit, in the fields of politics, of economics, of social life, or what you will, for the influences that flow back and forth across the northern border are as constant as the seasons and as persistent as the tides. A thousand pities it would be if the day ever came when we failed to profit by each other's successes, and take warning by each other's mistakes.

Now it is my very great pleasure to present to you the head of a great American institution. To introduce him to this audience would be a work of supererogation. I call on Dr. Farrand, of Cornell.

Dr. Livingston Farrand (Cornell University): Mr. Chairman and Members and Friends of the Society: The topic that has been assigned to me tonight was naturally suggested by the official explanatory phrase in the title of this Society. "The American Philosophical Society for Promoting Useful Knowledge" implies a process which common experience has shown to be freighted with problems of baffling difficulty.

I do not know whether or not there is any significant connotation attached to the adjective in the Society's subtitle. At any rate, I shall not attempt to discuss the differentiation between useful and useless knowledge. I am uncertain as to what the latter would be. I assume, however, judging from the historical account given by our toastmaster, that when the word "useful" was injected into the Society's supplementary title it had something to do with "practical" knowledge as we usually speak of it today. In other words, the incentive of application of new facts to industry and to human welfare probably bulked large in the minds of our predecessors who associated themselves in this effort to further the discovery of new truth and to spread the knowledge of what organized inquiry might bring to light.

This idea naturally suggests one of the ever present problems in our university development and administration, and that is the provision of adequate opportunity for research in every field of human knowledge. I make no apology, therefore, in bringing a word of congratulation to this Society on the one hundred and fiftieth anniversary of the granting of its charter for calling attention to certain of these problems which have come so prominently to the fore in our higher educational system and toward the solution of which it seems to me the Society can make contribution.

We may assume that every university recognizes its responsibility as a center for the training of the youth who flock to its halls. It is not always so clearly apprehended that the provision for organized research along the broadest lines is an indispensable factor in any higher educational foundation. At the same time, this point of view can be regarded, I think, as needing no defense and, if we are to be guided by experience, the vitality of a university is directly proportional to the vigor with which the spirit of inquiry is evident within its walls.

In recent years there has been a striking development particularly in Germany and the United States, of research in the fundamental physical sciences under the auspices of industry as distinguished from the university provision, and which presents one of the most encouraging efforts of our day. We must recognize, however, that, welcome as these new industrial laboratories may be, there are certain problems of coördination of effort which their presence inevitably presents.

It was only two years ago, I think, that I attended a gathering of distinguished figures in science and education to discuss the effort organized by the National Academy of Sciences for the raising of a large sum of money—some twenty millions—which should be utilized for the promotion of research in pure science; and the thought was advanced by some that in some way or other the achievement of that purpose might constitute a menace to the development of university research in science. Personally I could not and cannot see the danger,

but the discussion suggests a place of usefulness which it seems to me the American Philosophical Society can occupy.

I used a moment ago the phrase "coördination of effort" and this coördination has in recent years been much in the minds of those responsible for the development of scientific inquiry in our American universities. It was inevitable that, organized as we are in our educational foundations, the search for truth should become departmentalized as has the administration of our institutions. It is unfortunate but unavoidable. The result has been what might have been expected, an isolation of effort even within the walls of a single institution with the resultant loss of stimulus which closer association would obviously offer. What is true of a single center is much more striking when the great number of university and industrial laboratories are taken into account. In other words, while admitting a possible difference in immediate aim between industry and university, there is no intrinsic conflict in the processes of research and there is no reason why the development of one should not contribute to the strengthening of the other.

The practical difficulty is in affording an easy, obvious and constant avenue for mutual comparison and coördination of effort and it is here that it seems to me an organization like the American Philosophical Society has a very real function to perform. It would seem to offer a completely disinterested forum for the discussion of those problems which are a common concern of so-called university centers as well as of American industry and of American life.

The great advances in industry in this country which have been the direct result of the application of laboratory discoveries to practical affairs are so obvious and the material response so striking that the popular imagination has become excited. Wealth is lavishly poured out to provide research facilities and the public has been brought to a state of mind where it expects that new truth and new generalizations can be purchased at a price. The contemporary newspaper press in responding to what it conceives to be the demand of its public seizes with avidity on anything in the nature of scien-

tific discovery and day by day the public curiosity mounts. It matters not that the field of the new discovery may be so complex as to be understood by none but a small body of masters; the very unintelligibility of an announcement seems to increase its "publicity" value. As we sit here tonight there is eager and wide spread popular discussion throughout the land of the discovery of a new planet, the significance of which is certainly not clear to the eager and ignorant debaters but the fact of which stimulates imaginations even though those imaginations are not particularly well in hand.

While one cannot help deprecating the mistaken and unchecked expectations as expressed by popular reaction, the interest has its advantages which it is well to nurture and encourage. Aside from the material support for fundamental research which that interest affords, I am inclined to think that out of the experience is likely to come an increased respect for expert as opposed to uninformed opinion and I doubt if there is anything more necessary today in our sprawling American civic life. Certainly an application of the habits of the laboratory to, say, the problems of legislation would produce a marked improvement in the annual grist of statutes which emanates from our halls of legislation, federal and state.

I said a moment ago that university research in the sciences had become departmentalized and that to an unfortunate degree. Every forward looking institution is now endeavoring to concentrate and synthesize its efforts and to break down the artificial lines of division which are often nothing but administrative devices. Who can sharply differentiate today between the fields of physics and of chemistry or of either of those fields from biology, whether animal or plant? The very development of modern medicine described to you this evening is a striking example of the convergent contributions of all the fields of scientific inquiry. One of the greatest contributions that could be made to the breadth and soundness of public knowledge would be an appreciation of the necessity of unity of attack in the search for truth.

I close with the point with which I started, that it seems

to me this Society offers one of the best means we have for a comparison of effort, for a candid judgment of results, for an indication of values which can bring the industrial and the university worlds into closest harmony; that it is for that reason that with all my heart I bring congratulations upon the completion of one hundred and fifty years of recognized service to human thought in this country.

Mr. Davis: The last speaker, gentlemen, on whom I shall have the pleasure and the honor to call, is in many ways an educator himself. I think it would be only fair to his modesty to say that perhaps he doesn't rate himself so large a figure in the educational world as the distinguished gentlemen to whom we have just listened, but he cannot repel the assertion that he is both a successful lawyer and a distinguished diplomat.

There is one particular task which I wish to assign to him. I am told that in the one hundred and fifty years of its charter life, and the two hundred and three years of its real existence, this is the first gathering ever held under the name of this Society outside its birthplace. I think that it is only fair that those members of the Society who do not enjoy the inestimable privilege of being citizens of the City of Philadelphia should have a Philadelphian explain to them the reasons for this institutional immobility. I shall ask the Honorable Roland Morris of Philadelphia to report to us concerning the intra-Philadelphia activities of the Society.

Mr. Roland S. Morris (Philadelphia): Mr. Chairman, Guests, and Fellow Members of the American Philosophical Society: I am here tonight to answer the questions of the Chairman only because, unfortunately, Dr. Dercum, our President, as he said in his telegram, has been ill and is unable to meet you personally on this very unusual occasion.

It is quite true that, in what we Philadelphians like to call the two hundred years and more of the Society's existence (because we trace its ancestry directly back to the Junto which was established by Benjamin Franklin in 1727 in Philadelphia) this is, as far as we know, the first gathering of the members and guests of the Society outside of Philadelphia.

It is not because we of Philadelphia wished that: but rather because the habit of doing so was early established—first in the days of the Colonies and subsequently, after the adoption of our Constitution, the habit of holding our meetings in the building of the Society in Philadelphia was continued throughout the period of the Society's existence.

In this building adjoining Independence Hall and contributed by the City of Philadelphia, the Society has met on regular stated evenings on which occasions scholars and scientists have presented the results of their studies and investigations which were subsequently published in the proceedings of the Society. For many years it has been the custom of the Society to hold a general meeting each Spring and to bring together in the hall of the Society representative scholars and students of Science from the whole membership, for the purpose of presenting the results of their studies and investigations, and of summing up the year's work in their various departments. An attempt has been made during all those years to do precisely what Dr. Farrand has suggested as the opportunity of the Society now—and that is, to coördinate knowledge coming not only from the students of research themselves, but also from the practical knowledge and problems brought from men in active business, who were also chosen as members of the Society to make their contribution, so that every department of knowledge, as far as possible, could be represented in the membership. This was the original idea, and the one that we have tried to carry out in the succeeding years so that every department of knowledge might be represented by someone who could speak with authority on his particular field of study and investigation. So, by this method—this bringing together and blending of mind and thought in the development of knowledge—we might have opportunity for fuller coördination, a larger outlet and a firmer basis.

I think it is most interesting for us to realize, as we celebrate this anniversary, that we have had a series of proceedings dating back some years before the origin of our own Government; and that those papers contributed by its members and

published by the Society have been sent to the centers of learning throughout the world, during the century and a half that they have been continued. Thus the American Philosophical Society, through its publications and its opportunity and the length of time in which it has carried on its work, has attained a position, and a name, and a tradition in learning which is a source of pride to its members and an honor to the memory of Franklin.

In view of the development in the field of education (as we have heard it described by Sir Robert Falconer) and in the field of research (as we have heard it outlined by Dr. Livingston Farrand)—research by our great industrial organizations, by our universities and by the great foundations which have been referred to, all earnestly devoting their means and their efforts to the enlargement and extension of knowledge and its coördination. In view of all these educational and scientific activities and accomplishments Dr. Dercum and his colleagues and Dr. Scott, who preceded Dr. Dercum as President of the Society, have been asking themselves and the membership in recent years "what can we do, what can the American Philosophical Society do to assist not only in the enlargement and coördination of knowledge but also in its distribution."

What contribution can an organization like ours, with this fine tradition and history to conserve, make in the coming generation not only to justify itself, but to be worthy of those traditions, and to accomplish in this day and in the succeeding days what it did under much more simple conditions in the past?

And that is what I want to tell you in just a word at the conclusion of our dinner this evening, that is, the vision we have, and that Dr. Dercum and his predecessors and his colleagues have been presenting to us, and what we are trying to do to realize it.

We feel that the Society has come to the point where in order to meet the demands of the present, it should have better equipment; also a better home; so that it may enlarge its scope and bring together from time to time representatives in all fields of knowledge to enlarge the distribution of its printed

proceedings among the various institutions of learning and departments of research; and meet the enlarging field and opportunity that is offered to us today.

We have an amazing collection distributed in safe deposit vaults, in fireproof cellars; and thousands of books not even available because they have to be put in places where they will be preserved from fire; I say we have an amazing collection of almost priceless historical documents that has been developed and enlarged during the period of the Society's existence.

We have to find some adequate place for those documents and historical exhibits that have been accumulating from generation to generation, and which really constitute the symbolic story of the development of scientific thought in the United States during the last century and a half.

So the Society as a whole felt the time had come when we ought to have some proper housing. And almost entirely through the interest and contributions of members of the Society, many of them right in Philadelphia, we have already succeeded in raising funds to the extent of over a million dollars for a building to house our collections, and in which to conduct our activities.

We feel we should have just as much more before we finish our task, for the purpose of enlarging our personnel—not that we should do research work, that is being so well done elsewhere; not that we should have a faculty, or go into the field of education, but that we should be the clearing house, to keep in touch with all these faculties, foundations, research departments, great industrial research laboratories, the representatives of industry, representatives of different fields of learning who are members of the Society, so that we might practically accomplish what Dr. Farrand pictures to us—a coördination of knowledge; and give the stamp of the American Philosophical Society to the accurate knowledge so demanded by the mass of our people, and on which imagination is playing so vividly as discovery after discovery is made.

We feel we have a great opportunity to do just that work, and no other, and we can probably do it with an endowment about equal to the amount we have already collected, or a little more, so that we could have secretaries whose time shall all be spent in the interest of the Society—a sort of a secretariate; a sort of league of knowledge—with a group of men exchanging views, and thereby coördinating knowledge in various departments, and applied to numerous problems; and thereby make a contribution of real service to our community.

Now that is our dream, and it is realized, as I say, to the extent that we are ready to erect our building as soon as we have the endowment that would maintain our personnel. We have asked our members, and they have responded gloriously and generously to carry out this idea; and I confess (though as a very humble member of the Society, and with no claim to any such learning or achievement as would justify me in claiming any place in its membership) it thrills me to think how we can, in this rather simple way, and with a comparatively small amount of money, by virtue of the background and tradition that we have, help to coördinate all this mass of knowledge that is being acquired by that devoted band of men and women throughout the United States, who are giving their lives to research—whether in the laboratories of our great corporations today, or in our universities.

And just think how consistent this would be with the ideals of our founder, and with the tradition of our Society. If I may say this in conclusion, just picture the conditions of knowledge on this continent when Franklin called his first group together and organized the Junto, that they might confer from different points of view and as they gathered information they might exchange that information among themselves, and thereby make it helpful to youth.

And then think of the intervening years, as the Society quietly—I think I can say, modestly—year after year has gathered to exchange information on practically every great discovery that has characterized the progress of science during the last century and a half. We could show you in the min-

utes of the Society, in the little instruments that were displayed there, the very earliest beginnings of some of these great discoveries that have actually revolutionized our modern life and given us our modern conditions.

Now we come to a time when we have these great foundations, and these university research staffs, and these great industrial corporations, all seeking for the truth and trying to enlarge the bounds of knowledge, until the average man becomes perfectly bewildered. He doesn't know what is truth and what is not; what implications are sound and what are not; how he can coördinate; how he can adjust his own personality to all these modern complications and all this modern knowledge; and is not that our field?

I think that is not an impossible dream. I think it is going to be realized. And so on this anniversary as we look back and give thanks for what has been done, do let us look forward to the promise of the future. I think the first touch of that promise has come tonight when we have stepped forth from the parochialism of Philadelphia, Mr. Chairman, into the wider field of New York.

ADDRESSES MADE AT THE ANNUAL DINNER OF THE AMERICAN PHILOSOPHICAL SOCIETY HELD AT THE BELLEVUE-STRATFORD HOTEL, PHILADELPHIA, SATURDAY, APRIL 26, 1930, JOHN H. FINLEY, PRESIDING.

PRESIDENT DERCUM: Ladies and Gentlemen—This evening I have a very pleasant duty to perform. My function is a brief one. In a large sense, it is unnecessary, because I am going to introduce one who has kindly consented to act as toastmaster who is already well known to you. He has had a most varied personal experience and he has endeared himself to countless persons. He has among other things manifested an interest in this old Society of ours which could only have been inspired by an affection on his part and by an interest which we are very glad indeed to have. He will act as toastmaster, and will introduce in turn the various speakers. Their number is not large, but I know that you will enjoy to an unusual degree the things they will say to you.

May I introduce Dr. John Huston Finley.

DR. FINLEY: Mr. President, Distinguished Guests, Ladies and Gentlemen: When I was asked to act as Toastmaster here tonight, under the presidency of Dr. Dercum, I replied to the Secretary that the three speakers announced were all stars, and that while I was made, as I assume of the same star stuff as they are made of, I am only a vagrant planet, or possibly a comet, not a brilliant comet, but one that might be mistaken for a planet. Or it has occurred to me that I may, after all be only an asteroid. At any rate, I am not a fixed star in the philosophical-biographical firmament. But one is proud, sir, to be among the "star-scattered" guests of this Society, even for a single night and though my status is as uncertain as that of Object "X," the trans-Neptunian object to which I, rushing into print immediately, sug-

gested the name "Minerva" should be given, because it has sprung full-panoplied from the mind of man;—and it did seem to me, incidentally, also that Venus ought not to be left alone with all those gentlemen on the Olympian heights.

I wish to express to you sir (Dr. Dercum), while you remain here, my appreciation of the great honor you have done me, and especially the exceeding courtesy you have shown me in placing me in such a distinguished position [between Dr. Dercum and Dr. Elihu Thomson]. When in Scotland recently at a meeting of the Royal Scottish Club, I found myself similarly situated between two men of great distinction, and I said that I felt very much as a valley must feel between two mountains. And one of my neighbors, on rising to speak (it shows how quick the Scotch are in response, and how familiar they are with the Bible) said, "I was thinking when Mr. Finley was speaking of the verse in the Bible which says, "And the valley shall be exalted." I am very proud, indeed, as the valley to be exalted even for only a few minutes.

Our attention tonight is especially directed to the need of the coördination of the sciences under the leadership of philosophy, which has been called "the science of sciences," and more particularly under the auspices of the oldest of American scientific societies which it is suggested in this little book that you have before you might well be called "Aristotle, Incorporated." I have often likened the modern editor to the philosopher, a generation the senior of Aristotle, Democritus, "the Laughing Philosopher," as he has been called. Here is a brief summary of the interests and achievements of that philosopher: He was abreast if not in advance of the astronomy of his day; he had, as he himself said, of all his countrymen "traversed the greatest part of the earth"; he wrote a treatise on navigation, was learned in physics, writing on the Magnet, the Rays of Light and the Water Clock; he was "fond of music and poetry," leaving works on Rhythm and Harmony and on the beauty of epic poems; he was a critic in matters of art; he must have been a physician since he left a book on Fever, another on Prognostics, another on Pestilences, another on the

Right Way of Living; he assumed to write authoritatively on such varied knowledges as Agriculture, Tactics, the Principles of Laws, the Calendar and Colors, Ethics and, finally, on Cheerfulness, besides being a zoölogist, an anatomist, and a psychologist. One must know more to be even an editor, if he is to be a good one. Aristotle himself had a still wider range than had Democritus, but even he could not compass all of the known of our age. We have come to realize, sir, that what no one man can know must be attempted through the perfect communication of the few or the many who do know. Through the flock, the swarm, the herd, Nature is now seemingly hoping to achieve what the massing of innumerable cells in the one grand creature the dinosaur, the baluchitherium—I hesitate to venture into this field in the presence of Professor Conklin, but I have written it here and I will go on—could not attain. And it is so with the intellectual achievement of men. It is the day, sir, of the conference table in politics, of combination in business and of coördination in science.

I regret that the sun, if I may continue my astronomical figure, that the sun of our solar system Dr. Dercum, must depart. You will remember that tomorrow is the beginning of Daylight Saving, and the sun of our system must be on duty early in the morning. We hope that he will rise refreshed, and join us next year at the great annual meeting.

I don't know how I shall get on as a planet without the visible sun, but I shall proceed even in darkness.

Many years ago I read a statement by Professor Conklin to the effect that physically man had not surpassed the highest excellence of ten thousand years ago and that in intellectual capacity he had not advanced beyond that of a few men who lived two thousand years ago. And as I remember, he intimated that man had come to the end of his evolution. But I have remembered, too, what Mazzini said: that the soul was bound to mould for itself such a body as its needs require and have often wondered why our mortal clay were lifted to such heights if man can never reach the higher height. But I think your explanation was, and perhaps with that we must be con-

tent, that science can lift the many of the human race to the height, the maximum height yet reached, of the few. And it is in that hope that we persist, though I still dare to entertain the opinion that we may find it possible to go even beyond our present maximum reach.

I have the pleasure to introduce Dr. Conklin, of Princeton. I was introducing Dr. Shapley the other night, and I began by saying that he came from Missouri, which indicated that he was born into the scientific attitude of mind. When he rose to speak, he said he thought he would better continue the biography; and remarked that he had two older brothers who didn't want an education, so his father sent them to Princeton.

I suppose that was a long time ago, when I myself was a professor at Princeton. I will not admit that the accusation was even then justified but even if it were true, I would rather live in Princeton than be educated. I envy those who still are connected with Princeton University and are permitted to live there in that ideal, idyllic environment.—I present Professor Conklin of Princeton.

PROFESSOR EDWIN GRANT CONKLIN: Mr. Toastmaster, Distinguished Guests and Members of the Society,—Ladies and Gentlemen: When our Toastmaster referred to my formerly expressed views regarding the limitations of evolution and the excellence of ancient man as compared with modern man, I was reminded of the fact that he had in a sense misunderstood my point of view. I recall very well the beautiful poem entitled "Duovir" which he wrote in reply to my claim that in certain lines evolution had run its course and in which he expressed the aspiration for ever-continued growth and development, which must of course inspire every human heart.

Progress is, after all, the thing which makes life inspiring and desirable. It more than anything else makes youth glorious, for we are then in a period when we realize our own progress. Indeed immortality would be unendurable if it were not associated with endless progress. I am a believer in progress, and I want to set myself right with Dr. Finley on this point. My thought is merely this, in the course of organic evolution

progress has from time to time turned into new paths. For example, evolution in the one-celled organisms went as far as it could in the direction of complexity and perfection of a single cell; it reached a limit beyond which it did not and could not go, for there is a limit in every direction to the possibilities of development or progress; and then evolution turned into a new path and began to build up multitudes of cells, making more and more complex organisms, adding together not only cells but also tissues and groups of cells and body segments until the organism, the single person, became an enormously complex individual. These aggregations of simpler units led to animals like the giant dinosaurs and titanotheres that progressed in the direction of great size as far as they could go. I think that so far as mere size is concerned organisms have already gone as far as they ever will go. They have exhausted the possibilities in that one direction.

And then came this path of evolution which man has so successfully followed, namely the development of intellect. Many persons think that intellect is somehow released from the limitations of evolution which apply to the body. But after all, the intellect is intimately bound up with the brain, and if man is to have a greatly enlarged intellect in future ages, it must involve a larger and more complex and more perfect brain than that of the present race. And the question at once arises, will such a development be a symmetrical one? For a living organism is like a tightrope walker crossing Niagara gorge. It must maintain its balance. It must maintain a proper balance between its muscular, alimentary, nervous and sensory systems, between its physiological and psychological functions. It is very easy to throw the organism out of balance, and we see already a great many evidences in highly-civilized life that many men are getting slightly out of balance because of this strenuous exercise and development of the brain; but there is no satisfactory evidence that modern man is much in advance of many ancient men in either cranial or intellectual capacity.

I often say to students, who think that we must be vastly

wiser than the ancients, "Do you really think that Aristotle or Plato or Socrates would have been unable to pass the entrance examinations to Princeton? Would they be unable, if they sat on the benches with you to get through the courses with a passing grade?" After all, the more we know about the ancient Greeks, or even the Cro-Magnon race, the more we realize that they had very respectable minds. Doubtless many of them would have been elected to the American Philosophical Society if the Society had existed in those far-off days.

I have been looking over this evening, as you have, this remarkable publication entitled "Servants of Mankind." No doubt it suggested the topic which was assigned to me only a few days ago,—"Science in the Service of Mankind." Everyone of these words,—"Science," "Service," "Mankind,"—refer to another path of evolution upon which man has entered, namely social organization. It is through society, through social coöperation, that civilization has come to the stage of development which it has now reached and there is every reason to look forward to greater developments in the future. There can never again be another individual Aristotle but as we have learned from our Toastmaster, both in his editorials and in his address this evening, an "Aristotle Incorporated" is possible and is the thing which the world now needs. This is the new path of evolution upon which mankind is advancing. The great developments of the past five or ten thousand years have been in the realm of society. Man alone, of all creatures that inhabit or have inhabited the earth, is capable of conserving the past experiences of many generations and of bringing them to bear upon the problems of the present. All our culture, all our science, all our philosophy, is built generation after generation upon the work of those who have gone before. Every generation stands on the heights that have been built for it by those who preceded it, but every individual begins life where his ancestors began, namely in the valley of the germ cells; he climbs to the summit of maturity and goes down into the valley of the shadow of death. But

society, gifted with endless life, passes on with giant strides from mountain top to mountain top. And so it happens that society and social developments have outrun individual development and so it happens that books and articles are written on "The Old Savage in the New Civilization," "Puritan's Progress Away from the Celestial City," "The Sins of Science," "Science the false Messiah," etc.

One of the latest of these articles is by my dear friend and colleague, Dean Gauss of Princeton, in the May number of Scribner's Magazine entitled, "The Threat of Science." That article made me decide before I came here tonight not to attempt to give a catalogue of the services of science to mankind,—an absolutely impossible thing for me, and one that would be entirely beyond your endurance. What I think might be worth while saying is something with respect to this aligning of humanists against scientists. I have been a good deal perplexed, I must admit, to find out just what this "new humanism" is. The only definition I have found that seems to me clear and precise is this: "Humanism is anti-naturalism." Those who hold this view, say, "Science consists in measuring *things;* it cannot deal with *man*." The view is widespread that man is outside of nature, above nature, that the laws of nature do not apply to him. Of course, these humanists admit that science works so far as man's body is concerned. It can deal with the tangible and ponderable things of man's nature but not, they say, with mental, moral, æsthetic and spiritual values.

But science is more than "measuring things"; it is *knowledge* based upon experience which is verifiable; it is organized and accurate common sense. Surely broader humanists would not deny that this broader kind of science can deal with human values, that it can deal with mental and moral disorders no less than with physical ones. Indeed the measure of the intellectual and social progress of any age is the degree to which men regard all phenomena as the results of natural causation. All that these humanists mean, so far as I can understand, is that there is another aspect of human nature

than the scientific one. Man indeed can be looked upon from two points of view. There is the objective point of view, in which you regard a human being as an object, a specimen, a phenomenon; this is the scientific, the causal aspect. There is the other point of view, just as true, just as important, the subjective, the emotional, the religious aspect of man. And these two are not mutually exclusive, and neither of them is unnatural. They are both natural, by which one means lawful, orderly, regular, or, as Bishop Butler, the author of "The Analogy of Natural and Revealed Religion" put it, "That is natural which is stated, fixed, settled." And the phenomena of mind, of society, of science, of culture, of art, of literature, of religion, these are things which are natural; they happen according to principles and laws. I think in this connection of a wise saying of an old teacher of mine, "The idea of the supernatural or the contra-natural, is a mistaken one. Everything that is, is natural." Which merely means that everything is lawful.

Well, science is engaged in attempting to find these laws, these principles, that underly not only outside nature, but human nature. It recognizes the fact, as men have recognized it without any organized science, that it is through social coöperation that progress is possible. This is the new path of evolution; this is the direction in which progress has been going forward at a tremendous rate; this is the direction in which it will continue to go for no one knows how many ages to come. There may in time be some other path of evolution, but surely it is true that given indefinite time we will find that certain paths become blocked, and then if we have enough ingenuity and adaptability we will perhaps find new paths. What organism of the Silurian period could have anticipated or have dreamed of the developments that have come to pass in the world of today? No more can we forecast the possible paths of evolution in ages to come.

Science has not developed human nature to the same extent that it has developed our control over the inanimate forces of nature. It is a very much more difficult task to

change the natures of men than it is to capture the lightnings from the skies or to utilize these immense forces of nature that are hidden in chemistry and in physics. But I don't despair of science accomplishing much in the improvement of human nature. We know that there are two fundamental principles that are involved in the development of the human personality. One of these is found in the environment, in societies such as this, in education, in churches, in all the developments of culture, art, religion, science. These are environmental factors, and they have a tremendous influence upon the development of the individual. More fundamental than the environment is the material upon which the environment acts, and that we speak of as the nature, the constitution, the heredity. It is relatively easy to get at the environment and to change it; it is much more difficult to get at the heredity and to change that. We have done it, however, in the case of many domestic animals and cultivated plants, and it isn't hopeless to look forward to an improvement in human nature. When that time comes, as it surely will in a crowded globe, when the pressure of population causes society to take more thought as to the quality of populations, I think we will find that science will help us materially to a better type of human nature than it has in the past or present.

So I am in no wise despairing of the future of evolution. It hasn't stopped now, it isn't going to stop tomorrow. Throughout all the ages, it has been making toward a wider, freer, fuller life. I anticipate that it will continue to do so in all the ages to come. And where could we find a better meeting point for all the agencies that must coöperate in this advancement of mankind than in a society such as this? Man cannot live by science alone any more than he can by bread alone. Life is something more than mere knowing; it is feeling, willing, doing, as well as knowing, and we need all the aid of all the social agencies that make for progress in this attempt to control not merely the outside environment, but the natures of men; with all these forces and agencies focussed upon this problem, certainly the future seems to me alluring and brilliant.

Chairman Finley: Dr. Conklin, I thank you for this wonderful address. I shall myself go on through the rest of my life with a little more hope than I have had since I read that essay of yours a long time ago. It has been a transfiguring address for me.

And now I have the pleasure and the great honor to introduce, for the third time in three or four weeks, the gentleman who sits at my right. It was my happy fortune and distinction to offer Sir Hubert Wilkins,—he wasn't Sir Hubert then, he was Captain Wilkins—on behalf of the American Geographical Society, the first recognition that he had from America. An old professor in Princeton used to say, when anything extraordinary came to pass, that "it was almost providential." It was almost providential that the American Geographical Society had a gold medal in the making at that moment, the fund for the founding of which was left by the will of Samuel Finley Breeze Morse in 1872. It reached a size in 1926 which permitted the Society to devise a medal and make the dies, and it was voted to award it every five years for exceptionally distinguished work in geographical science. Sir Hubert landed on Saturday, the 21st of April. On the Monday following, this award was made upon him in Spitzbergen by the use of Morse's code.

Captain Wilkins might have rested on his laurels, for he was already among the immortal explorers, beginning with Ulysses, who however never got beyond the Pillars of Hercules. But Sir Hubert has gone from glory to glory. The Southern Hemisphere unites with the Northern in his praise. He has made straits of water where before there was only dry land. (The Almighty, you will remember, made dry land where there was water.) He has made islands out of peninsulas, so that in very truth, as Isaiah said, his praise is "declared in the islands." It may be said of him as could be said only of the Almighty in Job's time. "He looketh to the ends of the earth and seeth under the whole heaven, to make a weight for the winds" and to learn "the decrees for the rain"; for he is to undertake that, and in fact has already undertaken, that very

thing for the continents that extend their triangualr frames toward the South Pole.

I have a meteorological friend, Dr. McAdie, the head of the Harvard Observatory at Blue Hill, whom I have for many years called "the Anemocrat," the master of the winds. But the winds with whose governance he is concerned are the winds of the northern latitudes. He has only inter-hemispheric relations with those of the remote southern latitudes.

I salute this gentleman as the future Antarctic Anemocrat—the governor of the winds which blow cold from the South and warm from the North, and are in somewhat more savage state than ours. I say "governor," for to know is to control to govern,—as much as human beings can control by preparing for what cannot be averted.

There is a little temple of the winds at the foot of the Acropolis in Athens—dedicated to Zeus, the god of the winds. Some day there will rise an Antarctic temple of the winds to Sir Hubert, but till his great work is done, may even the ill winds blow him good, and may he, like Prometheus, come back from his snowy Antarctic Caucasus and dwell among men and especially among Americans.

Captain Sir Hubert Wilkins: Mr. Toastmaster, Ladies and Gentlemen—I wish to assure you of my deep appreciation both of the honor of being with you tonight and of the privilege of being one of those who go out into the fields seeking useful information. I realize, of course, that each one of you have been seeking and gaining useful information at this conference but I wonder if you realize how much there is to be gained from the exclusive field in which I have been working. Where you have been seeking you have had much company, a wide field and a great many sciences to choose from. Those of us who have of late become known as Polar explorers have been seeking particular information in company with only a few.

Those of us who are privileged to seek in the Polar regions today, have considerable advantage over those who went earlier into these regions, collecting as they did with great hardship and at great hazard, the fundamental truths that can only be obtained from high latitudes.

I found, when confronted with the possibility of addressing this learned society, that it was such a task that I didn't even attempt to prepare a speech. I thought I would just talk to you a while. But in listening to the wonderful speech made by Dr. Conklin, I have almost forgotten what I was going to talk to you about. The reason for that is, perhaps, because those of us who go out seeking useful information in the Polar regions do it in a more or less lonely manner. We seldom have the opportunity of taking with us books with which to pass away the time not occupied in active work, and seldom do we have the opportunity of joining with our fellow men, hearing their opinions and discussing with them the advances that take place. So, you can well understand my greediness for learning when I find myself at the feet of such learned professors, and you can, perhaps, understand why I have been tonight carried far beyond the memory of my own insignificant endeavours.

But I will try and explain to you something of the work that we have been doing, something of the reason why we seek useful information in the Polar regions. Those of us who gained our early experience with the famous explorers, Stefansson, Shackleton and Amundsen, have come to know from the work already done, the value of the work that is still to be done. And we find that the greatest work that we can do —the most useful knowledge we can seek in the polar areas is that in connection with the science of meteorology. I think you will agree with me that meteorology is one of the very great sciences—a world-wide science that must be studied not only from the point of view of a country or a continent, but also from the point of view of the whole world, if we are to understand the general mechanism of the weather.

We found, when we were studying the plan in connection with world meteorology that there were two geographical points at which we would need to make investigations, in areas not as yet surveyed. There was an area lying north of Alaska, and another lying to the south of the Pacific ocean, still uncharted. There were two large blanks on the map. So it became the duty and pleasure of those of us interested in

world meteorology first to do some geography and to discover the particular points from which to make our observations. We turned our attention first to the region north of Alaska and naturally, we looked for the most economic means of carrying out the work. We found that through the development of science in many of its branches—through the development of other methods of transportation than those used by earlier explorers, we could most profitably employ our time, and by using airplanes, in the Polar regions, pay great dividends to the science of geography. It had been stated by many people with limited experience, and it was believed by many, that it would not be possible to use aircraft in the Polar regions. There are always some who object to radical changes and improvement. But, I think, the work done by Admiral Byrd, Amundsen, Nobile and others has now proved to the world that the theories held by the inexperienced men in connection with polar flights were not quite correct. We have proved beyond doubt that we can use to advantage air transportation in the high latitudes. With the help of the airplane we surveyed the blank spot north of Alaska. We did not, however, find in that area the land we hoped to find and so we have been unable as yet to establish our meteorological investigators in that vicinity.

Having accomplished the work, with negative result, in the Arctic, we set out to investigate the region south of the Pacific ocean and I am pleased to say that our efforts last year and this year have been so successful that we are now in a position to formulate a plan of meteorological study in those areas.

The useful knowledge we can glean from those regions, will, we believe, help us to understand the mechanism of world weather and this knowledge must be gained before we can hope to forecast seasonal conditions with accuracy. Because there is still the need to discover some safe place at which to collect information needed from the Arctic, I think I might hurry on without relating any incidents that happened in carrying out our work so far and draw your attention to

our plan to introduce a new method of exploration—to the possibility of going one step further ahead than we can go by using airplanes.

The airplanes had proved their worth and their use has made a great advance in polar transportation. You can recall, most of you, the tremendous hardships and difficulties that were faced by Peary, Stefansson, Scott and others in carrying out their work in polar regions—you remember how they struggled day after day and year after year behind their dog teams. And you can understand—perhaps you will understand a little better if I relate just one incident—the advantage airplanes have afforded the modern explorer. One morning, last year, we left the breakfast table at eight o'clock, stepped into our airplane and by 8:30 had climbed to 10,000 feet. We flew for many miles over those high and glorious Antarctic mountains, surveyed more than 1200 miles of new coast line, discovered seven new islands, two channels, a strait, and a new part of a continent, came back, changed our clothes, had a hot bath and were sitting down to dinner at 6:30 that night. Because we have been able to use the airplane to such advantage and without the hardships suffered by earlier explorers you can well understand why we feel that we are not to be classed with them as heroes. With our planes we can cover two thousand miles a day, the early explorers who tramped behind their dog teams season after season, year after year, travelled five or ten miles a day—nor more than 2,000 miles a year. Yet they did a vast amount of useful work.

The airplane has aided us greatly in reconnaisance but I believe we can now move one more step ahead, and leaving the airplane behind, use a submarine for collecting useful information in the polar regions. With a submersible boat I believe we can approach areas seen from the air and from which we can gather information not only of value to meteorologists but which will be of interest to the people studying geophysics, terrestrial magnetism, geography, oceanography and other branches of natural science. The useful information we hope to gain in the polar regions does not only concern the

scientist. It is something that will be of interest to every one of us. Meteorology has to do with our pleasure when we set out for our morning walk, it concerns our food and our pleasure at the table, it has some connection with all the beautiful and important things of life and it has to do with many of life's terrors. A knowledge of it will we hope, enable us to avoid the consequences of droughts and famines now experienced in many countries. It was with the hope of avoiding the consequences of unexpected dry seasons that my attention was turned toward the investigation of the polar regions. I learned that with meteorological knowledge gained from the polar areas together with that collected in other latitudes we might be able to forecast seasonal conditions with accuracy. But I found that I was forced to be an explorer before becoming a meteorologist. We had first to do some geographical work before we could carry out our meteorological plan. After I had been for a number of years working in connection with meteorology, I learned one day at the Royal Society of London that one of my ancestors, Bishop John Wilkins, in 1648, had suggested that we turn to the Polar Regions for information which would lead to accurate weather forecasts. But I have learned since that he also proposed to do other things and it might interest you—since some of you, this afternoon, must have heard the address in which was mentioned the possibility of sometime reaching even farther afield than the poles of reaching even to the plains and hills of the moon—to know that this old bishop in 1648 so busied himself with mechanics and philosophy that he not only pointed to the possibility of learning the secrets of the weather from the polar regions but he devised a submarine, discussed the universal use of the steam engine, and explained to his fellow members of the Royal Society that there might be, in the future, a possibility of moving from this planet to another—to the moon, in fact. I might say, in this connection, that next week, in New York, there will be a little ceremony at which a book written about the possibility of going to the moon, by Bishop Wilkins, will be presented to a society which has taken the title of the

"American Interplanetary Society." How many members of that society ever expect to get to the moon I do not know but the knowledge that years ago people were dreaming of things which have now become established facts gives us some encouragement in the hope that even more of their dreams will eventually become realities.

I had hoped to give you some idea of the interest explorers find in polar regions and to have explained also why we seek useful information in those lonesome conditions. I am afraid that I have wandered from the subject but I can assure you that when we realize that many hundreds of years ago people were interested in the possible value of that work, believing that some day it would be done and that a society like this is interested in the work now being done, we become even more willing to do the work.

I feel much honored tonight to be associated with the people here present and to be accepted as a member of this society.

Chairman Finley: I myself made a phrase sometime ago which I see has been used by the American Philosophical Society, namely, "planetary consciousness." I see now that I shall have to go one step farther, to "inter-planetary consciousness."

In the Hebrew legends, there is an account of the ascent of Enoch to Heaven—before his final translation of which we have an account in the Scriptures. The story is that he was carried to the seventh heaven, the abode of archangels who not only arrange and study the revolutions of the stars, the changes of the moon and the revolution of the sun, "but also arrange teachings and instructions and sweet speaking and singing and all kinds of glorious praise." There Enoch was put under the instruction especially of the chief angel, to whom the Lord said, "Bring first the books from the storeplace and give a reed to Enoch and interpret the books to him." And so it was that for thirty days and thirty nights did the archangel instruct intensively. Think of the wisdom of an archangel who could talk, as it is said, "His lips never

ceased speaking for thirty days and thirty nights," while Enoch wrote down "all the things about heaven and earth, angels and men, and all that it is suitable to be instructed in," filling, it is added, 336 notebooks, which were carried back to earth and distributed from generation to generation and from nation to nation.

You are quite aware that I am introducing one who by name and office has succeeded on earth the instructor of Enoch, one competent to teach as much as any one human can the things about heaven and earth, angels and men, and all that "it is suitable to be instructed in,"—Dr. James Rowland Angell of Yale University, an archangelic promoter of useful and suitable knowledge.

Dr. Angell: In complying with the request to speak on "Promoting Useful Knowledge," I should wish to preface my few remarks with the platitude that all knowledge is potentially useful. The seemingly trivial and insignificant of today may become the indispensable of tomorrow. Nothing better illustrates this fact than Franklin's early observations upon electricity, which were originally regarded by most of his contemporaries as curious and amusing, but not likely to result in practical consequences of importance. The present day is justly enough spoken of as the electrical age, and the useless has become the invaluable.

The typical scientist is far more concerned to discover truth and to provide reliable criteria for its establishment and verification than to secure its general dissemination. In the long run, this is doubtless a desirable attitude to encourage, but society is profoundly interested in the distribution, as well as in the production, of knowledge, and not less in protecting its members from the false and the specious masquerading as knowledge. Quackery is by no means confined to medicine, nor are all of its most insidious evils to be found there. It is abroad in business, in industry, in political doctrine, and no area of human interest is wholly exempt from its incursions. It is incumbent on the intellectual leaders of our time to study this problem with every energy.

While as a people we have, in general, a well-grounded aversion to censorship, we must nevertheless recognize that modern technology has put at the disposal of irresponsible persons agencies for reaching the public mind such as no previous generation has known, and if these agencies may be employed, as they occasionally are, for the dissemination of grotesque falsehood, as well as of established truth, the public cannot be wholly oblivious to the consequences of this fact.

It has been truly said that the perfection of printing worked a revolution in the intellectual world. The perfection of radio and other electrical methods of communication is working an equally radical revolution. One need not be literate to hear over the radio an exposition of any subject whatever, and the effects of grossly misleading statements on matters of practical moment when thus absorbed by immature or ignorant minds, cannot be easily evaluated, but must be seriously harmful.

The newspaper press, the radio, the moving picture, the magazine and the book are the great means in our time of creating general social attitudes and of promoting general knowledge. The schools, the colleges and universities, the learned societies, give us the bedrock on which the larger part of what is enduring in all this process must be built. The church and the lecture platform no doubt play an important part, but, over the area of knowledge as a whole, probably far less than those agencies previously mentioned.

The only kind of control in this matter which can be counted upon to work is the power of truth to overcome error. But to accomplish this, we must rely ultimately upon the slow effects of education, education which shall in early childhood create a keen sense for the difference between knowledge and opinion, between demonstrable fact and sentimental desire, and an education which shall provide opportunity and inducement for men and women to continue their intellectual development and training throughout life, rather than leave it behind as finished and complete when the school or college gate clangs behind the outgoing graduate. This proposal

implies educational agencies of a kind materially different from most of those which we have developed in this country, although practically all of our existing educational institutions could be knit into a general system which would function as suggested. Whether it be done through the state or by private initiative, or both, there must be provision for the widest and most authentic possible dissemination of knowledge and that too of knowledge in all fields—not solely knowledge of physical science, and technology, and agriculture, important as these are, but also knowledge of social, economic, historical, political and ethical principles, with provision as opportunity offers for the cultivation throughout the entire community of taste and judgment and enjoyment in literature and the arts.

This is a large order, but anything less will leave us exactly where we are today, with highly imperfect methods for the promotion of sound knowledge and with lamentable inability to offset the too often cheap and fallacious material being poured out upon the people through the existing channels of publicity. The responsibility to think and act upon these problems devolves upon every intelligent person who desires to forward a saner and happier society, to see truth setting men free—and this particularly in the greatest democracy which history has known.

Chairman Finley: I will detain you just one moment,—first of all, that I may thank you again for the honor which you allowed me, to sit beside this oldest member of the American Philosophical Society—I mean in his membership—Dr. Elihu Thompson.

Only a few days ago—I hesitate to speak of this in the presence of these eloquent gentlemen—Thomas Mann, when he received the Nobel prize, remarked that "improvisation was a violation of esthetic economy." He said that immortal passages are often stricken off during these improvisations, as has been done tonight. But I dare not make any such venture myself.

I have two radiograms here, one from M. Poincaire, the

distinguished French statesman and member of this society presenting his excuses. And also one from Marconi, who expresses his great regret that he is unable to attend the meeting.

And may I just make one closing reference to Aristotle and to Alexander. We have talked about "Aristotle, Incorporated," but before this collective intelligence can be made permanently helpful to humanity, a modern Alexander must be found to endow it, as Alexander the Great, you will remember, endowed his great teacher, Aristotle.

I once went (it was during the war) to the village of Pella, only a few miles from Salonica, in Macedonia, where the palace of Philip of Macedon once stood. Nothing is left of all the ancient palatial splendor that must have surrounded the boy Alexander. But the $4,000,000 (I am told that was the value of the talents) with which Alexander financed his teacher has made him the conqueror of more worlds than he ever dreamed of. And he who endows this "Aristotle, Incorporated," has an opportunity to purchase a longer term of influence and fame than most of us dare hope for. I do hope, however, that before the next meeting of this Society, we shall find the Alexander who will endow "Aristotle, Incorporated."

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